The Information-Processing Theory of Aggression:

A Case Study In Scientific Development

Benjamin Hersh

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Helen Longino
Thesis Advisor

Robert Proctor
Second Reader

Department of Philosophy
Stanford University

"The returns to investments in children, especially investments in early childhood, in the first five years of life, are dollars very well spent, and reap returns at a rate of about 16 percent per year to the government itself—which is an investment we all would love to have in the stock market. The way that this works is that an investment in early childhood such as a program like Head Start or a nurse visiting the mother of a high risk family, or kindergarten and preschool education prevents costly outcomes later on, such as more children who have to go into special education, or more children who have to go to juvenile detention facilities... or prison... or become unemployed and go on to welfare such as food stamps... And, if you take into account the dollars that some children end up costing, those dollars can be prevented instead by investments early in life that reap great returns."
-Kenneth Dodge, 2010

"Every child knows that play is nobler than work."
-Judge Holden, 1849

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Introduction

In this paper I would like to accomplish two central tasks: 1) provide a thorough history of Social Information Processing Theory and its intellectual precedents, and 2) use that history as a case study to explore the limitations of philosophical models related to scientific development.

Introducing Social Information Processing Theory

Social Information Processing Theory is among the most lavishly funded and widely acclaimed research programs in the history of the behavioral and cognitive sciences.

Let me start in 1994: Nicki Crick and Kenneth Dodge, then of Vanderbilt University, published their paper "A Review and Reformulation of Social Information-Processing Mechanisms in Children's Social Adjustment" in the 115th volume of the *Psychological Bulletin*. This paper, cited nearly 2,000 times according to Google Scholar as of early 2010,[1] built a tenuous bridge between two disparate worlds of behavioral research: social psychology and cognitive science. Its title is something of a misnomer—we will see in later discussions that it hardly touched upon actual mechanisms of social adjustment, nor did it substantially reformulate earlier research. That is not to say that it was without value. Its real worth simply lay elsewhere—in its thorough synthesis of largely independent research fields and in the authors' unusual willingness to address their critics directly. Most of all, however, the paper served to popularize Dodge's previous line of research by re-introducing its theoretical core and dressing it in an updated scientific vocabulary.

In 1980, Kenneth Dodge created a research program that would grow into the Social Information Processing (SIP) theory of aggression. In 1986, he postulated a five step model to describe information processing patterns in children: 1) an encoding process, 2) a representation process, 3) a response search process, 4) a response decision process and 5) an enactment process. He maintained that delinquency—particularly in the form of aggression, was necessarily caused by problems in at least one phase of information processing. There was such a thing as an “aggressive child,” he assumed, and such children were shaped by information processing “deficits.” This line of research grew over the years, flooded into the world of public policy, and brought Dodge and his colleagues considerable success in their academic careers. Although the research has undergone many changes over the years, it has remained a continuous research program from its inception in 1980 to the present day.

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[1] Throughout this paper, I refer to the number times that various papers have been cited. I do this in order to give a rough sense of the scope of their readership and the degree to which the research community engages with them. Citation estimates come from Google Scholar as well as researchers' CVs.

Outlining The Project

In the first chapter, I will review the “pre-history” leading up to the social information processing research program. I will begin with a broad review of the cultural context surrounding the construct of delinquency. Next, I will provide history of how the modern notions of delinquency and maladjustment came into being, as well as some of the formal aspects of Dodge’s models. Then, I will review major psychological theories of aggression that preceded SIP research in moderate detail. These research programs vary in their scientific character, but nevertheless contribute to an understanding of where the roots of SIP theory lay.

The second chapter will concern the history of the research program itself, from its inception in 1980 through the present day. The study of any phenomenon as abstract and complicated as “aggression” makes experimental precision a difficult task, but we will see that other factors limited the research in much deeper ways, from poorly controlled experiments to uncritical peer review to highly selective attention to data. Nevertheless, we will see a number of internal changes in SIP theory that gradually shed many of its major weakness, and grows to gain many substantial strengths. The story of SIP theory is, in many ways, a story of scientific progress, and I intend to trace it through the stations of its redemption. My history will begin with a close reading of Dodge’s earlier papers and draw from representative samples of later research.

The third chapter will examine four central topics that arise in attempts to model scientific development: 1) units of analysis, 2) continuity, 3) maturation, 4) progress. I will use SIP theory as a case study to compare a range of philosophical models against these axes. The philosophical perspectives will include the classic models proposed by Thomas Kuhn, Imre Lakatos, and Larry Laudan, although I will also draw from Ian Hacking, Michel Foucault, and Emilio Metaxopoulos in certain cases.

I will conclude with a discussion of the lessons that may be gleaned from observing the relationship between philosophical models and the history of SIP theory. In this section, I will address broad methodological issues connected to philosophical thought, and discuss the limitations of the Cartesian epistemic tradition still alive in much philosophical work.

Methodology

It behooves me to warn the reader that the truth of the theories in question is ultimately of secondary importance to my philosophical discussion. In the footsteps of Hacking in his analysis of memory research and multiple personality disorder in *Rewriting the Soul* (upon which the present paper is very loosely modeled), I intend to relax my critical lens when investigating the historical stages of my subject, and focus it more sharply when it comes time to address the diversity of relevant philosophical perspectives.

The reader should also be aware of the interdisciplinary approach found throughout this project. The first chapter is largely a conventional history, and the third chapter is largely a conventional essay in the analytic tradition of philosophy. The second chapter, however, offers a variety of disciplinary perspectives in order to tell an informative and detailed history of SIP theory. It contains a chronology of developments within a particular body of knowledge, and addresses the social and intellectual context of that knowledge as well. It also contains the stories of people and institutions, as well as journals and citations. The sources range from interviews to academic papers to online resumes to data taken from Google Scholar and the Corpus of Contemporary American English. It is also worth noting that this is not a purely descriptive history of SIP theory—in places, I evaluate it against the methodological standards of contemporary experimental psychology. I make such evaluations for the sake of the broader normative issues at stake in the third chapter, and they should not be interpreted as a direct challenge to SIP theory.

1. A History of Ideas

1.1. Misbehavior Across the Ages

So long as western thinkers have codified their beliefs with regard to ethical and social questions, there has existed a legacy of expert wisdom pertaining to the correction of juvenile misbehavior and the conditions necessary to ensure that a child live virtuously as an adult. It is not my intention in this chapter to provide a comprehensive history of such opinions, or even a detailed outline, but only to situate contemporary work within in a broader cultural frame. Like the living carapace of a coral reef, our understanding of the world grows outward from an unseen ancestral skeleton and deep structures often underlie a textured surface; in the following pages, I aim to gesture at what lies below.

The Hebrew Bible, a document dating as far back as the fifteenth century BCE, seems like an appropriate place to start this investigation. As any reader familiar with the content or tone of the Hebrew Bible may suspect, the text contains rather harsh prescriptions for the correction of deviant behavior among children. Of particular interest are culturally resonant passages, such as Proverbs, 13:24, “He that spareth the rod, hateth his son; but he that loveth him, chasteneth him betimes.” Other passages reinforce this theme, such as Proverbs 22:15, “Foolishness is bound in the heart of a child; but the rod of correction shall drive it far from him,” and Proverbs 23:14, “Thou shalt beat him with the rod, and shalt deliver his soul from hell.” These passages (the latter in particular) set high stakes for proper child development, and establish a policy towards child delinquency and misbehavior that future generations, up to the present, day would turn to for guidance. One finds similar themes again in the New Testament, compiled a millennium later, in such passages as Luke 15:10, “The eye that mocketh his father, and despiseth the instruction of his mother, let the ravens of the valley pluck it out, and the young eagles eat it.”[2]

With the exception of Sparta, the ancient Greeks tended to de-emphasize punishment as a means of dealing with misbehavior. Both Plato and Aristotle anticipate the attitudes of 20th century social learning theorists, focusing on how children should be taught so as to develop properly. In *The Republic*, Plato outlines a curriculum for raising rulers of the state that emphasizes both comprehensive intellectual training and exposure to positive examples.[3] An improper education could have grave experiences for the conduct of a youth:

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[2] In finding biblical passages, I drew from Goldstein et al (1981), *In Response to Aggression*. My discussion throughout this chapter is close to theirs, although with a somewhat different focus.

> Suppose he has no contact with the Muse. Even if he did have some love of learning in his soul, it gets no taste of learning or enquiry, and has no experience of rational argument or any artistic pursuit. As a result, since it never wakes up and has nothing to feed on, and since there is nothing to purify its senses, it becomes weak, and deaf, and blind, does it not? ...Someone like this becomes an enemy of rational argument, I suspect, and an enemy of music and literature. He abandons any attempt at persuasion using rational argument, and does everything with savage violence, like a wild animal. He lives his life in ignorance and stupidity, without grace or rhythm. (Pg. 104)

Violent aggression, seen in this context as a failure to grasp rationality and culture, stems from a failure on behalf of the educators to properly stimulate and guide a child's mind. Ancient Greek education functioned largely to prepare citizens for their civic duties, and subsequently aimed to correct or prevent behavior that might impede rational discourse; thus, social aggression seems to have become an early "problem behavior."

The Middle Ages saw the rise of the doctrine of original sin; during this period, children were understood to contain an innate seed of evil within them. This evil could manifest as disobedience or aggression, and was met with harsh disciplinary measures in order to suppress it and ensure the child's ultimate salvation.

Eighteenth century thinkers rejected old medieval assumptions about childhood, with Locke famously likening the infant to a blank slate. Locke argues in *Some Thoughts Concerning Education* that children should not be beaten or punished harshly, but gently praised and blamed so as to reinforce positive behaviors and prevent negative ones.[4] Misbehavior represents neither an innate depravity nor a personal moral failure; rather, it is a malleable trait developed through life experiences in such a way that it may be prevented or corrected through the proper means.

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[3] Plato, pg. 92

[4] See pg 84, for instance

Rousseau took one step further away from the old medieval view. He argued that children are born with innately good instincts, but tend to be misguided by a depraved society. In *Emile*, he suggests a preventative approach, consisting of removing children from corrupt models and institutions in society. He remarks, "…I want to bring up Emile in the country, far from those miserable lacqueys, the most degraded of men… far from the vile morals of the town."[5] This attitude, as will become apparent, has its own historical echoes. The notion that misbehavior arises from an individual's fit within a problematic social framework, although not quite articulated as such by Rousseau, rises to considerable prominence in later years.[6]

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[5] Emile, pg. 59.

[6] Of course, the story does not end here. The 19th century saw a rise in deterministic Darwinian thinking, but that is less relevant to the ideas of the research tradition of interest. Similarly, I only discuss deterministic medieval views to compare them to later ones.

1.2 Maladjustment is Born [7]

During much of the 19th century, the prevailing view of mental illness and deviation had been that deterministic (and often biological) forces lay at their heart. Scientists such as Adolphe Quedelet had been exploring notions of "social physics," using newly developed statistical models to predict crime rates and understand deviations from the human norm. Individuals who were considered deviant or ill were isolated, without hope of recovery, and held in the custodial care of asylums. Mental illness was brought to the forefront of American culture when, in 1881, President James Garfield was assassinated by the mentally unstable lawyer Charles Guiteau. While mental illness had been heavily stigmatized throughout American history, this incident made it a dangerous trait in addition to a shameful one.[8]

Leading into the 20th century, American psychiatrists were trying desperately to maintain professional prestige.[9] Following its formation in the mid 19th century, the American Psychiatric Association had declined a merger with the American Medical Association, which was comparatively crude at the time. Following the rise of germ theory and the importation of European scientific medicine over subsequent decades, however, American doctors became effective and reliable healers for the first time, leaving psychiatry behind in the realm of methodological anarchy and etiological blindness. The failure of American asylums to cure the mentally ill encouraged younger psychiatric professionals to adopt clinical practices, seeking more mild ailments within their curative power. At the turn of the 20th century, young psychiatrists found themselves involved in the real world, confronting psychological problems in the streets, households and classrooms rather than the confines of an asylum's custodial ward.[10]

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[7] In this section, I draw heavily but by no means exclusively from Margo Horn's analysis in *Before It's Too Late*, 1989 and her 2009 course "A Social History of Mental Illness."

[8] Rosemberg (1995)

[9] Horn frequently refers to Rosenberg, C. 'The Crisis of Psychiatric Legitimacy: Reflections on Psychiatry, Medicine, and Public Policy," in G. Kreigman, RD Gardner, DW Abse, eds, American Psychiatry: Past, present and future (Charlottesville: university of Virginia, 1975)

[10] This is essentially a summary of Grob (1995) and Rosenberg (1995).

Perhaps the most pressing problem that the new generation of experts faced was that of juvenile delinquency. During the first decades of the 20th century, the public mind was filled with reports of crime waves and lawless, unsupervised youth. One editorial wrote

> "It seems to be an accepted axiom nowdays that our young people are going to the devil. Press, pulpit, and publicist are agreed that youth is wild and getting wilder. The college boy and his flapper friend, it is charged, drink, pet, and are disrespectful to their elders; while the neighborhood gangster, aided by his youthful sweetie and stimulated by the false courage of heroin or cocaine, robs and murders with casual calmness long before he is out of his teens."[11]

At this time, perceived delinquency and other allegedly immoral behaviors, particularly promiscuity in young women,[12] often resulted in either institutionalization or the intervention of social workers. That is to say, misbehavior in minors was often classified as mental illness, and thusly demanded sound medical, scientifically motivated treatments. Young psychiatrists and social workers were overwhelmingly left wing, and authors such as Horn generally depict them as working to medicalize and treat the social problems that seemed omnipresent in American cities. Juvenile justice systems were established in major cities (often containing psychiatric clinics), data was collected, and interventions were attempted.

Despite the best intentions of the psychiatric community, the etiology of mental illness (and especially catch-all disorders such as delinquency) remained rather opaque. Well into the 1900s, old-fashioned hereditarians such as Henry Goddard and Charles Davenport argued that delinquency was predicted by fixed, inherited traits—often traits along the lines of feeblemindedness or mental defects.[13] Such views were closely linked to eugenics, as subnormal mental faculties were understood to be the root cause of crime. Nevertheless, the general consensus was that delinquency was multicausal, and external causes became emphasized increasingly. Left-leaning psychiatrists and social workers dominated the field, and reformers of the 1920s looked to economic and social factors, noting that delinquents were often from poor or immigrant groups, had dissonant families and lacked positive examples to learn from.[14]

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[11] "The Commonwealth Fund Program for the Prevention of Delinquency," Staff Newsletter 32 (May 1926): 5, CF.

[12] See Lunbecks' discussion in "'A New Generation of Women': Progressive Psychiatrists and the Hypersexual Female."

[13] See Horn's comments, pg. 17. Horn cites Cravens (1978) Ch 2.

Like the etiology, issues concerning the proper treatment and prevention of delinquency were contentious, murky and sometimes sensational. One popular movement began in 1908 with the founding of the National Committee for Mental Hygiene. Issac Ray, a founder of the American Psychiatric Association, describes mental hygiene as follows:

> …the art of preserving the mind against all incidents and influences calculated to deteriorate its qualities, impair its energies, or derange its movements. The management of the bodily powers in regard to exercise, rest, food, clothing and climate, the laws of breeding, the government of the passions, the sympathy with current emotions and opinions, the discipline of the intellect—all these come within the province of mental hygiene.[15]

The notion of "mental hygiene" dates back to the work of William Sweetzer in the 1840s. It did not catch on in the Darwinian milieu of the 19th century, but by the early 1900s, thinkers like G. Stanley Hall began investigating ways to preemptively fight mental illness during childhood. The eminent "psycho-biologist" Adolf Myer became an ardent advocate of optimism concerning mental illness, and worked with the mental health reformer Clifford Beers to bring mental hygiene to the psychiatric forefront. The mental hygiene movement had profound effects in the mental health profession, one of which was that it further focused the attention of social workers and psychiatrists on behavioral deviance among youth. Delinquency, a catchall for activities like drinking, stealing and promiscuity, became a major subject of study in psychological research and the target of its applications.[16]

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[14] Horn, pg 13

[15] (Rossi, A., Some Pre-World War II Antecedents of Community Mental Health Theory and Practice. *Mental Hygiene*, 1962, 46, 78-98).

[16] Johns Hopkins University. (2007). *Origins of Mental Health*. http://www.jhsph.edu/dept/mh/about/origins.html

Throughout this period, significant demographic shifts occurred. Families were shrinking in size, and the birthrate had been on decline for decades; the median size of all households in the United States fell from 4.7 in 1900 to 4.3 in 1920.[17] Between 1900 and 1930, the percentage of the population under 15 years of age dropped five percent, and the median population rose from 22.9 to 26.5. Families consequently invested unprecedented resources into child rearing, and relationships within the family were "democratized" as rigid social roles gave way to more spontaneous interactions. Science carried tremendous social prestige during this period; parents embraced the advice of authoritative experts, thinking that they were the first generation to benefit from new scientific knowledge and insights into child rearing.[18]

Given the murky etiology underlying mental illness, it comes as no surprise that the optimism about social reforms and behavioral interventions was to little avail. By 1930, the child guidance movement had toned down considerably; ambitious efforts to protect community mental health had largely dissipated, leaving a core of professionals situated to treat the "problem child" at the heart of it all. The classic problem child had normal intelligence, but exhibited a range of behavioral and psychological problems that were categorized with the label of "maldadjustment." Such children were 5 to 13 years old, and typically surfaced in settings external to the family—particularly in educational and care institutions. These children had trouble responding to the immediate expectations held of them in their social settings, which were typically set by their adult caretakers. Behaviors that could be characterized as maladjustment included:

> [in younger children,] thumb-sucking, nail-biting, enuresis, masturbation, peculiar food fads, and night terrors, to personality traits such as sensitiveness, seclusiveness, apathy, excessive imagination and fanciful lying. Also included was a category of undesirable behavior in older children such as disobedience, teasing, bullying, temper tantrums, bragging or showing off, defiance of authority, seeking bad companions, keeping late hours, and sexual activities.[19]

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[17] Horn pg 36, cites Notestein 1931 and Glick 1977.

[18] Horn pg 38 .

[19] Horn, pg 39, references Ralph Truitt et al, The Child Guidance Clinics and the community (new York: Commonwealth fund, 1928), 11

The creation of this category served to separate delinquent youth from those suffering such severe mental defects that behavioral intervention would prove futile. Psychologists such as Meyer emphasized the interpersonal nature of the label, as is evidenced by a very literal reading of the phrase “mal-adjusted” (i.e., poorly adjusted to a group or social setting). Focus was turned to new possible sources of behavioral problems, such as the home environment or parenting styles, and the entire project of fighting delinquency was brought to a more local level. “Heroic” procedures (contrasted with psychodynamic ones) such as lobotomies and leucotomies were widely used in the 1930s to treat severe cases delinquency, and not wholly without success; Antonio Moniz, who developed and popularized the leucotomy, won the Nobel Prize for medicine in 1949.

The concepts of mental hygiene and maladjustment functioned to create an extremely broad notion of the at-risk child. There was considerable hope that many forms of mental illness, such as delinquency, could be treated, but one considerable obstacle remained: the etiology remained largely obscure. Numerous theories have been suggested between the 1930s and 1980, when Dodge’s research program begins. During the 1950s, for instance, the “Palo Alto School” of researchers modeled family dynamics to calculate possible causes of maladjustment—such research gave rise to the practice of family therapy. Researchers in the 1970s began to think about social “skills” as an integral part of social adjustment, and I will revisit this topic at a later point. Nevertheless, no generally accepted etiological account of maladjustment had surfaced over this 50-year period, and confusion ruled the day when Kenneth Dodge established his career as a psychology researcher during the early 1980s.

Throughout the period of time under discussion, delinquency has remained a perennial social problem in the United States and abroad. Although it was known to correlate strongly with poverty, its etiology at the psychological level remained unclear, to say the least. The 1950s saw a period of renewed fear, which subsided over subsequent years.[20] Delinquency was fought largely through social welfare and supervision programs during the 1960s and 1970s, but the pendulum subsequently swayed towards more putative measures and fearful attitudes. During the 1980s, delinquency lost some of its previous sexual connotation and became more strongly associated with both violence and drug use; between 1980 and 1995, arrests for violent crimes by children under 12 had doubled. Arrests for forcible rape went up 190 percent and arrests for weapon possession rose 206 percent.[21] Although delinquency remained a catchall for undesirable behavior, aggression was perhaps its most salient feature during the last two decades of the 20th century. The perceived stakes were high in the study of maladapted and at-risk children, and aggression lay at its heart. Similarly, we will see that for the first time in its long history, behavioral aggression research became nearly synonymous with the study of maladaptation and delinquency during the late 20th century.

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[20] Perhaps the reader will be familiar with cultural artifacts from this period in film and theater, such as *Blackboard Jungle* (1955) or *Westside Story* (1957).

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[21] These statistics were found at http://www.faqs.org/childhood/Co-Fa/Delinquency.html

1.3 The History of Five Steps

Recall from the introduction that the “core” model of the SIP research tradition is composed of five steps: 1) an encoding process, 2) a representation process, 3) a response search process, 4) a response decision process and 5) an enactment process. This model says nothing of how information is processed at any given step, nor does it provide many mechanistic insights at a broader level. Although Dodge attempts to distinguish between the sub-processes empirically, we will see that his success was generally a matter of degree. One might wonder why Dodge chose to describe information processing in five steps in particular, when three, six, or a dozen steps could probably work just as well for the purposes of a computational analogy. Is five simply a culturally significant number? Would breaking step two into multiple parts make the model too cumbersome to be of heuristic value? Years earlier, Dodge had worked with a three step description of SIP—was the original found wanting in explanatory power, or did it simply look unimpressive in print?

I will return to the issues raised above at a later point, but in the meantime I would like to turn towards historical antecedents. One interesting aspect of Dodge’s five-step model, proposed in 1986, is that such models did not exist in the earlier aggression research. Building on his informal three-step model, the 5-step model was an original contribution to the relevant domain of research, and although it certainly constituted a response to specific earlier models (as we will see later), it did not incorporate any of their formal features.

While it is possible that Dodge’s model was entirely novel, this is vastly improbable—especially when one turns to previous uses of mental chronometry in cognitive psychology. Mental chronometry, or the use of behavioral reaction time to ground inferences about cognitive processes, dates back to the work of Franciscus Cornelis Donders during the 1860s.[22] Donders, a prominent ophthalmologist, conducted an experiment measuring reaction times to visual stimuli. The experiment began with a simple reaction time task, in which the subject responds to a stimulus (e.g. touching a button when a light goes on). Next, a discrimination reaction task was added, in which the subject would need to respond conditionally to certain stimuli and not to others (e.g. touching a button when the leftmost light in an array goes on). Finally, a choice reaction time task was added, in which an additional task of choosing from multiple possible responses (e.g. multiple buttons, each conditional on a different stimulus). Donders assumed that time devoted to perception and motor responses (as measured by the simple task) were constant across every other task. Simple task reaction times were the shortest, followed by discrimination tasks, and then by choice tasks. Each additional layer of complexity in the task was interpreted as a discrete element, and with this assumption, it became possible to calculate the amounts of time necessary for various types of mental work.

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[22] Donders, F.C. (1869). On the speed of mental processes. In W. G. Koster (Ed.), *Attention and Performance II. Acta Psychologica, 30*, 412-431. (Original work published in 1868.)

Donder's methodology was deeply influential during the cognitive revolution a century later, and found its way into many subsequent research programs. Saul Sternberg, a cognitive psychologist, was particularly inspired by Donder's work, and in 1969 wrote *The Discovery of Processing Stages: Extensions of Donder's Method*.[23] This paper gave rise to a resurgence of mental chronometry during the 1970s, including work on memory processing and the rotation of mental images. Also in 1969, he released an iconic application of mental chronometry: "Memory Scanning: Memory Processes Revealed by Reaction Time Experiments."[24] This paper has been cited nearly 1000 times, and even if Dodge had not read it himself, he would have likely felt its influence.

Sternberg had studied contextual memory retrieval[25] and memory scanning previously[26] but his 1969 paper was notable for its attempt to build a detailed cognitive model of such phenomena through mental chronometry. His experiment was essentially an application of Donders' methodology to memory retrieval. Subjects would begin by holding a series of numbers in their short-term memory, and then perform additional tasks. For instance, a subject might be given a two-digit number to remember and then presented with a new number and asked whether the new number was in the original series. He used various string lengths, and found that reaction times grew predictably based on the number of digits in the original series. Using such experiments, and building his way up in the complexity of tasks as Donders had a done century before him, Sternberg developed a model to characterize what his experiments were measuring. In it's simplest form, it appears as follows: 1) stimulus encoding, 2) serial comparison, 3) binary decision, 4) translation and response organization. Sternberg goes to great length to clarify the relations between each step, and is quite modest in how he presents this work. This model is intended as a fairly straightforward analogue to what he measures experimentally, and functions as much to clarify his results as it does to explain them. That is, the does not commit him strongly to the existence of these discrete cognitive stages outside of his operational characterization.

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[23] Sternberg, S. (1969) The discovery of processing stages: Extensions of Donders' method. In W. G. Koster (Ed.), *Attention and performance II. Acta Psychologica, 30,* 276-315.

[24] Sternberg, S. (1969) Memory-scanning: Mental processes revealed by reaction-time experiments. *American Scientist, 57,* 421-457.

[25] Sternberg, S. (1967) Retrieval of contextual information from memory. *Psychonomic Science, 8,* 55-56.

[26] Sternberg, S. (1966) High-speed scanning in human memory. *Science, 153,* 652-654.

Sternberg's model bears some obvious similarities to that of Dodge, released 17 years later. Aside from the linear structure and vaguely computational vocabulary, the actual steps employed by the two models bear a striking resemblance. The fourth step of Sternberg's model even includes a conjunction, the latter half of which is equivalent to Dodge's fifth step. As we will later see, Dodge advertised his appropriation of work in cognitive science and it seems reasonable to speculate that this included some of Sternberg's legacy. Despite being a historical antecedent (and possible ancestor) to Dodge's model, we will see that the two models function in extremely different ways, and Sternberg's work may be interpreted as something of a methodological foil.

1.4 The Science of Aggression[27]

In this space, I will discuss a wide variety of historical theories regarding the nature and causes of aggression. These theories cover a multiplicity of conceptualizations of aggression, and many focus on entirely different aspects of the phenomenon. Towards the end of my discussion, I will offer an overview of the types of behavior that generally get counted as aggression, at least across some of the perspectives most directly relevant to SIP Theory.

1.4.1 Psychoanalytic Instinctual Theory

Though not particularly scientific in its methodologies, Freudian psychoanalytic theory made a profound impact in the American intellectual milieu. Initially, Freud explained aggression as a reaction to the interference of pleasure-seeking or pain-avoiding behavior.[28] As his views developed over time, he characterized human motivation as an emergent phenomenon, arising from the opposition between life instincts (Eros) and death instincts (Thanatos).[29] Actions occurred as a result of their interplay, and aggression was interpreted as behavior propelled primarily by the death instinct. Aggression, in this view, was not a reaction or a behavioral bi-product of some set of conditions, but an intrinsic force universal to all people. The death-instinct could be discharged either inwards or outwards, and thusly account for self destructive as well as cruel or sadistic behaviors.

Despite the widespread influence of Freud's work, his specific views regarding aggression were not particularly popular, even among advocates of psychoanalytic theory.[30] Aside from the uninviting cynicism inherent in the view, it stretches its credibility rather thin as a generalization about actual human behavior. In particular, contemporary behaviorist research provided powerful alternative explanations for classic examples of "inward" aggression (e.g. drinking, overeating) in terms of immediate rewards and automatic reinforcement processes. Freudians generally compromised Freud's views, holding that aggression arose from an instinctual drive, but de-emphasizing the role of inward death instincts. The general lack of positive evidence for any of Freud's instinct theory has done little to dissuade analysts from using it, but it has sufficed to vastly limit its appeal among scientifically minded researchers. As we will see, however, that is not to say that similar instinct-based theories were absent from among the more scientifically oriented schools of thought.

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[27] My analysis draws from Bandura's 1973 literature review in *Aggression: A Social Learning Analysis*, and unless otherwise stated, my references to Bandura will be to Chapter 1 of this book.

[28] See Freud 1920

[29] See Freud 1922, 1933

[30] Bandura 1973, pg 13

Freud's theory of aggression aside, it is also worth noting that psychoanalytic theory left a tremendous mark on both popular culture and less scientifically oriented fields. Patrick Suppes and Hermine Warren (1978) used quantitative methods to analyze a number of resources used in training teachers. They found dramatic increases in the number of pages explicitly devoted to psychoanalysis and derived concepts from 1940 until about 1960, where their sample ends.[31] They found similar patterns in educational psychology textbooks as well. They also looked at mental hygiene textbooks, and although there is a general drop in the number of pages explicitly devoted to psychoanalytic theory after 1920, they found a complimentary rise in the number of pages devoted to derived concepts.[32] I will not speculate that this pattern continues after their window of analysis ends (1910-1957). However, it does indicate a divide between the theories that teachers were being exposed to, and as we will see later in this chapter, the more scientifically oriented contemporary research programs. Teachers will play an important role in the experiments associated with SIP theory, so this theoretical division is worth keeping in mind throughout the next chapter.

1.4.2 Ethological Theories

Instinctual explanations of aggression received new attention from ethological researchers during the 1950 through the 1970s. Most notable among this generation was Konrad Lorenz, whose 1966 treatise *On Aggression* approaches the subject with a wide range of ethological and anthropological methodologies. He argues roughly that, as a result of natural selection, members of most species (and males in particular) are endowed with a fighting instinct. In humans, this instinct may be shaped and ritualized in sophisticated ways, but it reflects an internal force common across many creatures. While this aggressive instinct is internally driven, Lorenz believed that it could be "vented" through appropriate outlets (e.g. sports). Contrary to Freud's view, Lorenz's model of aggression does not oppose a rival "life instinct." Like Freud, however, Lorenz motivates his view largely through anecdotal evidence and observation rather than experimentation.

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[31] Suppes and Warren (1978) pg 344.

[32] Ibid. pg 366.

Although widely influential and popular among the lay audience, many of Lorenz's methodologies and conclusions have undergone the critical scrutiny of more experimentally and developmentally minded researchers. Bandura's literature review highlights Lehrman (1953), who wrote disparagingly of instinct-based nativism, on the ground that it discourages the study of insightful and experimentally manipulable developmental patterns, he argued. A number of classic studies demonstrate the power of controlled experiments to isolate developmental factors and lend support to Lehrman's attitude. For instance, Kuo (1930) famously used several experimental conditions to separate diet and other factors from learning history in the development of kittens, and showed that aggressive behavior (measured as hunting behavior) has its roots in the imitation of adult models. Even researchers in the field have been critical of many of Lorenz's views, and particularly of his hereditarianism.

Another substantial limitation is that while preformed habits may govern the behavior of many "lower" animals, humans are immensely malleable, with a tremendous potential for learning and a high degree of cognitive control that can potentially render particular innate instincts irrelevant to the study of human behavior. A number of research programs have investigated the hormonal basis of various behaviors in animals, and some researchers have made a case that hormonal control of certain behaviors decreases with advancing evolutionary complexity.[33] The literature on these and related issues is vast, but I will go no further in that direction.

1.4.3 Cognitive Control

Despite its flaws, the notion of a fighting instinct plays nicely into many theories of cortical control in cognitive neuroscience. The basic idea is that "newer" brain regions (particularly specific areas in the Prefrontal Cortex) function to moderate and inhibit lower level processes. Numerous studies have linked "older" subcortical structures to the facilitation of aggressive behavior. For instance, activation of the hypothalamus and adjacent regions in animal models has been demonstrated to be sufficient, though not necessary[34] for attack responses. Nevertheless, innumerable problems have arisen in attempts to localize the roots of aggression in specific regions, and no simple answer has arisen.[35] Cortical control, on the other hand—a process thought to inhibit inappropriate aggressive behavior—is relatively well understood. Again, there is a tremendous literature on this topic that lies well beyond my present scope, but many authors before Dodge's time did argue that problems in cortical control might be linked to aggressive behavior.[36] A classical example that the reader may be familiar with is the story of Phineas Gage, a railroad worker whose frontal lobes were damaged by a stray tamping iron in 1848, after which point he became irate and unable to hold jobs or maintain social composure. This anecdote captures the spirit of the findings in this area, although it goes without saying that the topic of cortical control is quite complex and, in many ways, a matter of speculation to this day.

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[33] E.g. Ford's 1951 *Patterns in Sexual Behavior*. Bandura references Beach 1969 along these lines as well.

1.4.4. Frustration and Aggressive Drive Theories

Among experimental psychologists and theorists, innate instincts have carried little credibility or explanatory power. One popular alternative was the "drive," which builds as a function of frustration and the interference of intentional actions. John Dollard and his colleagues first developed a set of testable propositions in their 1939 paper "Frustration and Aggression,"[37] and a research program was born. Researchers often disagreed as to whether aggression was a learned response to aggression or not, but it was generally assumed that frustration was necessary, if not sufficient for aggressive behavior to occur among humans.

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[34] On this point, Bandura 1973 cites Ellison and Flynn (1968)

[35] One classic example is Delgado's 1967 study, in which hypothalamic stimulation caused aggressive responses only in dominant monkeys, and elicited submissive responses in monkeys from lower hierarchical positions.

[36] E.g. On this point Bandura discusses that Mark and Ervin (1970) found that assaultive patients in clinical care are more likely to have abnormal electroencephelograms, which measure average activation across cortical regions. Similarly, Bach-y-Rita et al (1971) argued that assaultive behaviors could be linked to cortical trauma from head injuries or physiological deficiencies from impoverished backgrounds.

[37] I could not find a copy of this paper, but it is very widely cited and discussed in relative detail by Bandura.

Although widely embraced by researchers, drive theories were not without their critics. Some critics have emphasized the cultural diversity of frustration responses[38] and others have focused on the fact that children often become submissive rather than aggressive when frustrated.[39] Other researchers, such as Arnold Buss (1961) have demonstrated that only very specific types of frustration seem to elicit aggressive responses.

Difficulties in defining frustration have also undermined the research program, as frustration had been stretched to include a panoply of exemplars, from personal insults to physical pain to blocking goals in a game. Some critics such as Albert Bandura (1969) have highlighted the variability of responses to frustration due to differences in learning history or intensities. Due perhaps to its vague and overextended definitions, drive theories have failed to demonstrate predictive power as experimental designs have grown more demanding. During the 1960s, researchers found that without reference to external variables such as learning history, frustration could scarcely predict whether an animal would become more or less aggressive.

Many researchers rationalized these findings with reference to cognitive inhibition, which itself was believed to vary as a function of potential punishments. Thus, aggression could be understood as the result of competing environmental influences, and a conflict model developed by Neil Miller (1959) became the topic of study. Formal deductions from such models were tested, but generated only equivocal results. One result of the conflict model was the notion of displaced aggression, in which the object of frustration was a potential source of punishment as well. It has been argued that displaced aggression is responsible for racism and aggression against out-groups, although there is little empirical support for this claim.

Another consequence of drive theories is the popularity of the notion of cathartic aggression, which would function to discharge built up frustration. There is a general lack of empirical support for the efficacy of cathartic aggression, and number of studies have found that rather than decrease as the drive is released, aggression tends to increase after "catharsis." Subsequent researchers have addressed the problem of catharsis[40] but by the 1970s, the drive research program had passed its prime.

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[38] Bandura describes the work of Bateson (1941)

[39] Bandura discusses Barker, Dembo Lewin (1941), Wright (1943)

Drive theories were central to the development of subsequent social information processing research. Researchers such as Feshbach (1970) and Camp (1977) attempted to connect cognitive variables to aggressive behavior. Berkowitzz (1977) argued that the attribution of hostile intent on behalf of the frustrating agent was a necessary causal step in the relation between frustration and an aggressive response—this assumption, as we will see, figures prominently in Dodge's subsequent work

1.4.5 Deindividuation

Deindividuation approaches have their roots in the research of Festinger (1952). Roughly speaking, deindividuation research looks at the ways in which self-awareness and social accountability relate to behavior. The connection to aggression was made clear by Stanely Milgram's 1963 research, which demonstrated that a substantial percentage of normal test subjects could be brought to inflict life-threatening electric shocks to their peers through perceived anonymity and the introduction of authority figures to separate them from feelings of culpability.

Philip Zimbardo, of prison-experiment fame, followed up on Milgram's work and explicitly addressed themes of deindividuation in 1970. He argued that environmental factors that create anonymity separate individuals from their sense of self awareness and social accountability, and can thusly result in cruel and violent behavior. He focused his analysis on violent mobs and riots, as well as secret societies such as the Ku Klux Klan, who hid their faces while committing acts of aggression. In laboratory settings, he was even able to elicit cruel behaviors simply by covering the faces of his participants. In his infamous prison "experiment" in 1972, he assigned groups of Stanford undergraduates the labels of "prisoner" and "guard" and released them into a role-play scenario in his department's basement. Within days, students had assumed their roles so fanatically that the experiment was brought to a halt for fear of serious injuries being inflicted on the prisoners by the guards.

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[40] e.g. Feshbach (1970)

Zimbardo’s research, although perhaps reckless and poorly controlled, contributed to the strongly “situationist” milieu of experimental psychology.

1.4.6 Social Learning Theory

Growing out of a behaviorist experimental legacy, Albert Bandura was a founding father of social learning theory. Like other behaviorist approaches, it emphasized learning and made few theoretical commitments to internal forces or drives that could not be operationalized experimentally. Essentially, it stated that aggression is transmitted through observation and imitation, not unlike a meme. This approach applies primarily to cases of instrumental aggression (i.e., aggression as a tool), but can also account for seemingly unmotivated aggressive behaviors. His research from the early 1960s demonstrated that children who observed violent adults were vastly more likely to behave violently in turn. His famous 1961 “bobo doll” experiment documented this nicely; in this study, children imitated adults’ interactions with a large doll, replicating their behavior in cases of gentle play, verbal abuse, and physical abuse. Despite (or perhaps because of) the striking simplicity of this study, it proved deeply influential and gave way to further research on what became known as “social learning.”

While many authors have contributed to the literature on social learning, its greatest champion remained Bandura, who in 1973 released the monumental book *Aggression: A Social Learning Analysis*. This book systematically reviews the literature of aggression research up to the point of its publication, and sets the agenda for a new generation of research. Bandura and other researchers proceeded to study various modes of social learning, including transmission through television and the observation of role models. Social learning research has continued through the present day, and newer findings have served to reinforce the notion that humans are imitative creatures and that we learn most of the problem solving strategies in our behavioral repertoires through social learning mechanisms. In this view, an aggressive child is one who has learned to behave aggressively, either in order to obtain a goal or even due to mere exposure to aggression in their environment.

1.4.7 Social Skills

Researchers such as Harvard's Robert Selman (1976) connected variations in aggressive behaviors among children to differences in social cognitive skills. Selman and other contemporary authors hypothesized that inappropriate aggressive behavior was a function of a lag in the acquisition of role taking skills. Such theorizing diverged from the minimalism and extreme empiricism of social learning theory, although the emphasis on developing social skills makes it a close relative. This research anticipates Dodge's later articulation of "social information processing deficits" although Dodge treated it as an alternative hypothesis rather than a theoretical basis.

1.5 What Counts as Aggressive

1.5.1 Among Researchers

The category of human behaviors that we intuitively recognize as “aggression” is sufficiently abstract and complex as to present a serious challenge to any researcher hoping to draft a precise or universal definition. Nevertheless, scientific rigor is a matter of degree, and researchers have generally settled for ways to define aggression somewhat reliably in the absence of sufficient and necessary conditions. In formulating drive theory, Dollard and his colleagues (1939) defined aggression as “any sequence of behavior, the goal response to which is the injury of the person toward whom it is directed.” Bandura discusses subsequent researchers, such as Berkowitz (1962) and Feshbach (1970), added “injurious intent” as a necessary condition.

There are numerous forms of aggression in which the affliction of suffering appears to be either secondary or totally irrelevant to the subject’s intentions, and theorists have struggled to account for such behaviors—often by specifying more specific types of aggression to fit their evidence. We will see this pattern in Social Information Processing theory as well. Bandura (1973) characterizes the concept of aggression present in most of the psychological literature as “behavior that results in personal injury and in the destruction of property. The injury may be psychological (in the form of devaluation or degradation) or physical.”

1.5.2 Aggression In a Broader Context

It is important to make a distinction between the behaviors that researchers choose to delimit as aggression and what people actually call “aggression” in ordinary social contexts. The subject of how people perceive and recognize aggression in others is of tremendous importance to this line of research, although relatively few researchers before Dodge’s time sought to answer this question empirically. Bandura and Walters (1963) noted that the intensity of a person’s action (e.g. the loudness of speech) was correlated positively with attributions of aggression. Bandura (1973) also notes that expressions of pain or injury from the recipients of aggression make third parties far more likely to label an action as aggressive. Similarly, assumptions about a person’s intentions can make the same behaviors appear either injurious and aggressive or merely clumsy.

David Holmes (1968) found that individuals are more likely to ascribe attitudes to others that they themselves posses. Although his experiments did not investigate why this occurs, they suggest that individuals who often behave aggressively may be more likely to perceive the behavior of others as aggressive—an important finding to keep in mind in later discussions. Similarly, people with a low threshold for pain perception may classify a wider variety of behaviors as aggressive, relative to people with a high pain threshold. This is supported, if somewhat speculatively, by contemporary work in visual perception. Toch and Schulte (1961) designed a clever binocular rivalry experiment (i.e. measuring the results of automatic visual processing), demonstrating that after three years of training in a law-enforcement course, police administration students became measurably more perceptive to violent images.

Bandura (1973) emphasizes that factors such as an observer's socioeconomic level, gender, ethnicity, education, occupation, and so forth, are obviously relevant to how one labels an action. Different groups of people abide by different behavioral norms and hold different expectations of others, creating a plurality of normative standards by which one might appear aggressive. Similarly, the gender, race, or social status of the person being observed may be highly relevant. Assertiveness among women may seem more aggressive than among men, and dark-skinned gang members may appear aggressive to sheltered suburbanites regardless of their actual conduct. For this reason, Bandura clarifies that "aggression is characterized as *injurious and destructive behavior that is socially defined as aggressive on the basis of a variety of factors, some of which reside in the evaluator rather than in the performer*."[41] Social scientists are no more immune from their own biases than are lay people—although researchers generally attempt to measure relatively specific notions of aggression, one must recognize the practical impossibility of strict scientific objectivity on their part.

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[41] Pg 8.

2. Social Information Processing Theory

2.1 Humble Beginnings 1980

1980: Kenneth Dodge was listed as a paper's primary author for the first time in what would become a prolific academic career. As a graduate student in Duke's clinical psychology program, he had only appeared as a co-author on Sharon Gurwitz's 1975 and 1977 papers "Adult's evaluations of a child as a function of the sex of the adult and the sex of the child" and "The effect of confirmations and disconfirmations on stereotype-based attributions." Although he did not publish any of his own research while a student, Dodge won substantial recognition for his academic promise before completing his doctorate in 1978—including a Duke fellowship with the National Institute for Mental Health and a National Research Service Award. Fresh out of graduate school, he worked at Duke University briefly before accepting a tenure track position at Indiana University, where he could set the terms of his own research.

2.1.1 Motivating Research

"Social Cognition and Children's Aggressive Behavior," Dodge's 1980 paper, represents a very clear intersection between the worlds of mental health and drive theory. He frames his research as the application of concepts from the literature on cognitive development to the "problem of inappropriate and persistent aggression among certain children." Such brazenly value-laden language is not uncommon in the world of delinquency and aggression research, but does stand somewhat at odds with the vocabulary typical of contemporary strands in developmental and cognitive research. Dodge draws heavily from the drive theorists, as he presents aggressive behavior as a failure to inhibit inappropriate aggressive responses to frustrating stimuli.

Dodge begins by limiting his study to the notion of "defensive aggression," which he characterizes as "aggression which is a hostile and assertive response to perceived threat or intentional frustration." This is differentiated from "instrumental aggression," which he characterizes as "injurious behavior intended to gain an independent reward and which may be altered by the appropriate manipulation of reward and punishment." This distinction has various precedents in previous research, and will resurface as an issue of concern in the social information processing research program.

Dodge further frames his study with the contrast between two competing accounts of defensive aggression: “cue utilization deficiency” and “cue distortion.” Cue utilization deficiency refers to a lag in the ability to integrate information about another person’s intentions. That is, a persistently aggressive child may suffer from a cognitive shortcoming that prevents them from properly grasping whether a peer’s intention is hostile or helpful, and this results in inappropriate responses. Cue distortion is the hypothesis that the aggressive child perceives intentions in a distorted way that biases their expectations towards the attribution of hostility in their peers. Dodge remarks that the likelihood of misperception increases with the ambiguity of the stimulus. With this in mind, he sets out in this study to test the competing hypotheses by studying how aggressive children respond to ambiguous cues. Of course, this presupposes that ambiguous cues will not increase the likelihood of mis-integration (Dodge never explains the motivation for this assumption), so there is some ambiguity regarding what Dodge is really in the business of testing.

2.1.2. Experiments

In his first study, Dodge looked at 90 boys, grades 2-6 from a rural lower middle class school. The students were broken into two samples: aggressive and non-aggressive. In order to separate these samples, Dodge had 326 students nominate their three peers who fit behavioral descriptions pertaining to aggression (e.g. “This child upsets everything when he gets in a group?” [sic]). Children were then asked to nominate three peers who they liked the least and the three peers that they liked the most. Scores for these nomination categories were computed as a simple sum of nominations for each child. Teachers were then asked to privately assess each of their students on a scale of 1 to 9 in terms of their initiation of fights, social relations, and total involvement of fights.

Children who were placed above the median of teacher aggression ratings and below the median of peer favorability ratings were classified as “aggressive.” This pool was predominantly male, and Dodge selected only males for the final sample. Dodge made the decision to select only males in order to ensure that the samples were representative of the pool, rather than on the basis of the children’s’ actual aggression ratings. One third of the sample was black, which paralleled the overall school composition, and reduces possible confounds with race-related observer biases. “Non-aggressive” or children were selected using the same method of comparing teacher and peer evaluations, and, represented the most liked, least combative group. This paper does not disclose the racial composition of the "non-aggressive" sample, which leaves open the question of whether racial composition and related observer biases affect the results.

The basic experimental design was that a child would be exposed to a frustrating negative outcome in a puzzle task. The child would be completing a puzzle with a pre-recorded, anonymous and unseen peer in the next room, simulated through audio information. The child was told that a prize would be awarded for the puzzle's completion, but inevitably the peer would break the puzzle and disappoint the child. The researchers conducted the experiment in a special trailer and executed it through a clever arrangement of microphones and speakers. Each sample of subjects was divided into three experimental conditions:

1) The hostile intent condition, in which the child would hear his peer say, "Gee, it looks like he's got a lot done. Well, I don't like it. I don't want him to win that dumb prize, so there, I'll mess it up. [Crashing sounds are heard.] There ... that'll do it."

2) The benign intent condition, in which the child would hear his peer say, ""Gee it looks like he's got a lot done. I think I'll help him put some more pieces together. Hey, there's one. I'll put it here. [Crashing sounds are heard.] Oh, no, hey, I didn't mean to drop it. I didn't mean it."

3) The ambiguous condition in which child would only hear "Gee it looks like you got a lot done. [After a long pause, crashing sounds are heard]."

The children were videotaped and later watched by two observers who independently coded their behavior along seven measures of aggressive and cooperative behavior.[42] Children were coded based on whether they displayed aggression at various stages of the experiment, and any disagreements between observers were resolved to the point where they agreed about every instance of aggression.

Dodge found that about half of both the aggressive and non-aggressive children responded to the hostile condition with aggression, and that very few of either group responded to the benign condition with aggression. Dodge emphasizes that aggressive children were more likely than their nonaggressive peers to behave aggressively in the ambiguous condition. As is always the case in statistical research, the researcher's discussion focuses on a strategically selected subset of the data. While there is in fact a relative difference between samples, as Dodge claims, his tables reveal that neither sample was particularly likely to behave aggressively in any particular situation. For most measures of aggressive behavior, only about 20% (and as few as 13%) of the aggressive children behaved aggressively, and there is no measure of that behavior's intensity. The weakness of these results does not receive attention here, but we will see that subsequent papers not only acknowledge this issue, but use the weakness of the findings as a special type of positive evidence for their theory.

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[42] "(A) disassembled one or more pieces of the other's puzzle, (B) expressed verbal hostility, (C) showed indirect hostility (such as hitting the wall, pounding the table, or making a fist), (D) assembled one's own puzzle, (E) attempted a neutral communication with the other child, (F) made a positive verbal statement, and (G) helped assemble the other's puzzle."

In contrast with Dodge's expectations, the aggressive children were more likely than non-aggressive children to help their peers after the puzzle was broken. Moreover this was a difference of 33% across groups, which equals the most extreme difference with regard to aggression in the ambiguous condition. As Dodge himself acknowledges, this precludes the possibility that the aggressive children were blindly aggressive, and demonstrates a greater degree of sensitivity towards their peers than one would expect given the manner of selecting the samples.

Using the same samples, Dodge produced a second study. In this study, Dodge asked children to imagine one of two scenarios involving negative outcomes: one involving milk spilling at a lunch table and one involving being hit by a ball. In each story, a peer is responsible for the outcome, and their motivation is left ambiguous. Dodge was hardly naïve regarding observer biases in attributions of aggression and hostility, and he took this into account by using the names of peers that the subject had previously labeled as either likable or unlikable. The experimenter then asked the child about the intentions of their peer, and recorded their statements.

Sure enough, "aggressive" boys were more likely than "non-aggressive" boys to attribute aggression to their hypothetical peers. Interestingly, older aggressive children were more likely to have hostile biases, which suggests that whatever factor separates the two samples behaviorally has a greater effect over time. Also, as one might expect, both aggressive and non-aggressive conditions were more likely to attribute hostility to peers that they had previously judged to be unlikable. Dodge takes these results (particularly the former) to be robust support for the hypothesis that increased intentional ambiguity would increase the likelihood of social misperception, although his discussion does not make clear that the difference across samples is relatively small. In the coded aggression scores, the average difference between aggressive and nonaggressive samples is about .1. By comparison, the difference in coded aggression within either sample and across the scenarios involving friendly and unfriendly peers is generally about .4. Across both samples and conditions, the coded aggression ranges from 1.03 to 1.86, which makes it clear enough that the sampling accounts for relatively little of the variance in the results, and about a quarter of what observer biases seem to account for. Dodge remarks that the observer's biases were significant, but his discussion decidedly focuses on differences between aggressive and nonaggressive children. Any difference across conditions lends a degree of support to the experimental categories, and in the study of phenomena as complex and subtle as human aggression, that should not be taken for granted.

Dodge states that he as twice demonstrated that ambiguity in social cues increases the likelihood of misperception among aggressive children, but not among nonaggressive children. He takes this to support the theory of cue distortion over its competitor, the theory of cue utilization deficiencies. He also links his work to the notion of a feedback loop—that children who are perceived as aggressive will be treated aggressively and thusly respond aggressively, contributing to a vicious cycle. This possibility seems plausible enough, and finds some support in his finding that differences across samples increase as a function of age.

Most researchers' first papers fade rapidly into obscurity, but this one did not. It has been cited 815 times according to Google Scholar[43] and was republished as a classic in the field in three anthologies since 1980—most recently in 2008. Nevertheless, the paper bears a number of methodological weaknesses, some of which are universal to nearly all behavioral research, and others that are particular to this set of studies. Abstract categories such as "aggressive," for instance, create difficult problems for any researchers hoping to anchor their work in well-controlled experiments. On the other hand, the first generation of studies conspicuously lacks a distinct control condition, which one typically expects to find in psychological experiments of any kind. We will see that subsequent studies do, for the most part, include conditions that function as controls. Since the final samples include only the 14% at either extreme of the school population on a popularity scale, there is no clear middle ground—an issue that later papers will address within a decade. Given the paper's sparse theoretical motivation, it is unclear why ambiguous cues would affect one group and not the other—the answer to this question, as we will see, develops gradually, and over multiple decades. Despite the various problems attached to these studies, Dodge did succeed in finding a measurable results, and the very existence of these hard-earned results seems to have sufficed to encourage further research.

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[43] Although this is certainly an impressive number of citations, one must not make the mistake of assuming that all of them are appropriate. Baron-Cohen (2002) cites it as a reference to support his claim that boys are more aggressive than girls—a minor point that is altogether secondary to Dodge's investigation. Dodge neither cites any sources nor provides experimental evidence when making this claim.

2.2 Groundwork: 1981-1985

Dodge was quick to follow up on his 1980 results, and released a second paper in 1981. This paper was "Biased Decision-making Processes in Aggressive Boys," co-authored with another recent PhD. Joseph Newman, and cited 295 times according to Google Scholar. It uses Dodge's earlier work as its primary theoretical motivation, although it does cite a few other contemporary authors.

With reference to prior research, Dodge and Newman remark that aggressive boys exhibit a "bias towards attributing hostility to peers in unwanted circumstances." Building directly from Dodge (1980), this paper extends its framework to approach two previously unaddressed issues pertaining to cognitive processing in aggressive boys: 1) speed of decision-making and 2) selective recall of hostile cues. Speed of processing was of particular interest, and the researchers explicitly clarified that they meant to draw from the information-processing notion of "perceptual readiness," or "priming."[44] The basic assumption underlying their approach was that aggressive children would be primed to behave aggressively and therefore process less information before selecting behavioral responses, even if the information was contradictory to their attribution of hostility. It was assumed that as a consequence of this, aggressive children would also be less likely to recall information that they had been exposed to.

Dodge (1981) uses the same methods as in his first paper to select a sample of "aggressive boys" and a sample of "nonaggressive boys" from among a student body. The problems of this method aside, it appears as though the researchers were able to select comparable samples to Dodge's earlier study, which should not be taken for granted. The basic design of the study was that each child subject would a) listen to a story about a boy who may have committed a certain act, b) gather information that might make it clear if the boy in the story did the act, c) state whether the boy had in fact done the act. If he decided correctly, he would be rewarded with a prize.

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[44] Priming is a robust and thoroughly studied topic across many sub disciplines in cognitive science, from language to visual processing. It can be measured in a number of ways, and has many unforeseen results. For instance, in one provocative study Bargh et al (1996) found that subjects primed semantically with words associated with old age subsequently walked more slowly than those exposed only to age-neutral terms.

Some of the stories involved hostile actions[45] and the child would be given the opportunity to hear the "testimonies" of other characters. For instance, a person saying "I saw him running in the direction of that woman's house after school." The number of testimonies that the child would choose to listen to would be taken as a measure of decision-making speed. This is not a direct measure of processing speed at the level of priming or perceptual readiness, as these phenomena are generally thought to occur at an automatic, low level of processing. Rather, it presumably rests on the assumption that if the children are primed towards attributing hostility, they will be "more ready" to come to a conclusion without going through the steps of a more rational approach. The child would later be asked to freely recall the testimonies after the experiment, which was taken to be a measure of the selectivity of their memories.

Dodge and Newman found that aggressive children were in fact more likely to listen to fewer testimonies than their nonaggressive counterparts. Their results, however, were not particularly dramatic, as neither group listened to more than an average of two testimonies per story, out of a possible five. The difference across groups is said to be that aggressive boys listened to 30% fewer testimonies; with such coarse coding, however, this finding begs for further exploration. Surprisingly, there was no significant difference across conditions in terms of what decision they make in the task. Although the researchers do not discuss this, it does suggest that whatever apparent differences occur at the level of cue perception or recall processes have little or no bearing on behavior. Moreover, it can even be interpreted to mean that "aggressive" children exhibit more efficient (and perhaps, effective) social cognition, to the extent that they process less information but perform at the same level of social intelligence on the "testimony" task. This is essentially the opposite of what the researchers assume throughout their analysis.

Across both aggressive and nonaggressive groups, a majority (about 60-70%) of the subjects decided that the children in the stories were behaving aggressively. This means that the apparent difference in the number of testimonies attended is not necessarily matched by a significant difference in behavioral outcomes. In terms of recalling information selectively, they found weak but significant correlation coefficients across both groups when comparing the likelihood of attributing hostility to that of remembering testimonies selectively. However, the correlation coefficient for the aggressive group was substantially greater than that of the non-aggressive group (r=.35, r=.17).

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[45] E.g. an old woman complaining that a child spilled her groceries and ran away. She could name a suspect, but was not certain. All of the stories followed this type of structure and were intended to contain ambiguous elements.

Once again, Dodge (1981) ties this research in to the notion of a feedback loop that creates a vicious cycle of aggressive attributions—what begins as a mild predisposition to react abnormally to ambiguous social cues may cascade into a disruption of the social order in a classroom or playground. This downward spiral would occur as a function of events at the interpersonal level, and any serious account of the situation must look at more than a single unit within the fray. However, there remains a remarkable asymmetry in the approach thus far: both sets of experiments test only deterministic models of behavioral problems originating from within aggressive children, and do not explore environmental or social factors. If the pathology of aggression were fundamentally a matter of what occurs at the social level, as Dodge himself claims, it is peculiar that Dodge would only consider models that locate aggression exclusively within the problem child.

It seems that Dodge was perfectly aware of the asymmetry in his research, and following the initial studies in the early 1980s, we begin to see an effort to broaden the scope of his analysis. This occurs most significantly as a brief trend in 1982, when Dodge published five papers that sought to expand his methodological framework to encompass theoretical entities beyond of the "aggressive boy."

The first such paper that I would like to address arose from Dodge's collaboration with fellow professor at the University of Indiana at Bloomington, N. Paul Brakke, and Duke professor John D. Coie. This was "Behavior Patterns of Socially Rejected and Neglected Preadolescents: the Roles of Social Approach and Aggression," and it has been cited 231 times according to Google Scholar.

Unlike many of the studies that I will discuss, "Behavior Patterns" 1982 is not a study of the aggressive child per se, but one of social status more broadly. Dodge and his colleagues used very similar methods of sample selection as in 1980[46] and labeled either extreme group as "rejected," in the case of what may have formerly been described as "aggressive," and "popular" in the case of what was may have formerly been operationalized as "non-aggressive." A third, "average" group was identified as that with the mean scores on peer ratings. Each sample consisted of only 8 children, which made it logistically possible to observe them in more naturalistic settings.

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[46] They appear to have used the same set of questions to acquire peer evaluations, but did not use teacher evaluations to select their samples.

The study consisted in the observation of the children over several periods of 5 minutes in the classroom as well as social time outside of class. Observers coded their behavior on the basis of predetermined categories of inappropriate behavior. For instance, the category "*Solitary-Task-Inappropriate*" consisted in behaviors that appeared to be a "violation of classroom norms.. [such as] clowning, daydreaming, and aimlessly walking around the room."[47] These criteria resonate strongly with the concept of maladjustment characterized in my previous chapter, and hardly seem to have changed over time.

Social behavior and peer interactions were also coded into four categories. Of these, two were aggressive acts. Aggressive acts were defined as "verbal or physical hostile or destructive behaviors that are assaultive, taunting, obstructive, or threatening in nature and are directed at a specific peer." This includes physical hits, showing, pushing, name-calling, teasing, and berating a peer. It was also noted whether the target subject was the initiator of aggression or the object of it.

Four female graduate students took observations, and there was relatively little agreement between them. Whereas the previous studies had 99 to 100% agreement across observers, it came out to only 68% in this study. However, given the vast differences between naturalistic observation and observation in laboratory settings, this drop in agreement may have been unavoidable.[48]

Within the scope of agreed-upon observations, the researchers found a number of measurable, if not dramatic differences between the groups. For instance, it was found that both the popular and rejected groups engaged in inappropriate behaviors significantly more often than the average group. The rejected group was also characterized by frequent attempts to initiate social contact with peers, but was also more likely to be rejected. Overall, Dodge and his colleagues seem to have discovered that rejected children are more likely to be rejected by their peers, and that popular children are less likely to be rejected. While perhaps banal, this finding lends substantial credibility to his operational definitions for different social groups—at least when he refers to them as "rejected" and "popular."

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[47] Pg. 393

[48] 68% may not appear particularly impressive, but it is not a particularly weak result in the context of other work in related fields. For instance, McCrae et al (1987) investigated correlations between personality assessments made by various judges and the targets' self descriptions, and found correlation coefficients of about .4 across a number of dimensions.

Dodge and his colleagues sought to replicate their findings in a second study, in which they controlled for race and gender, modified the coding system, and most importantly, introduced a category of "neglected" children.[49] The neglected category were those who were neither liked nor disliked by their peers, and who were deemed to have low social impacts by the researchers. It was found that neglected children were less likely than any other group to approach their peers, and otherwise the findings from the first study were essentially replicated. The neglected group behaved the most appropriately when alone, and the other groups were almost uniformly indulgent in inappropriate behaviors when children were observe individually. In social situations, the rejected children were the most likely to behave aggressively towards peers. Perhaps more importantly, however, rejected children were substantially more likely to behave inappropriately when interacting with their teachers; this was matched teacher assessment, which identified rejected children as the most aggressive.

"Behavior Patterns" goes hand in hand with another 1982 paper, "Dimensions and Types of Social Status: a Cross-Age Perspective," by Coie, Dodge and Heidi Coppoteli, also at Duke University. Although this paper is cited an impressive 1,173 times according to Google Scholar, I will only touch upon it briefly.

The researchers approached sociometic status as a function of two independent dimensions: 1) social preference and 2) social impact.[50] The researchers analyzed peer evaluations from schoolchildren, and found a number of significant correlations. For instance, social preference correlates positively to cooperativeness, supportiveness, and physical attractiveness. On the other hand, social preference correlated negatively to both disruptiveness and aggression.

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[49] This category is appropriated from previous studies, one of which I will discuss later in this section.

[50] Social preference was determined with reference to children's nominations for their most liked peers. Social impact was measured as the sum of a child's liking score and their disliking score.

Coie and his colleagues divided groups of children into five categories, using the same methods as above: popular, average, neglected, rejected, and controversial. The "controversial" category was introduced by this study, and characterizes children perceived to be disruptive (akin to the rejected group) but also to be social leaders (akin to the popular group). The researchers do not indicate whether these categories overlapped, but for the purposes of the experiment, this seems unlikely. After interpreting peer evaluations among school children, it was found that these categories could be dissociated from one another reliably. Figure 1) displays the relationship between the different groups along the dimensions of social preference and social impact. It is worth noting that the authors do not seem to regard the controversial children as a particularly vulnerable group. Whereas the rejected children are considered to be "at-risk," it is less clear that the controversial ones ought to be, given their perceived "social impact" and leadership roles. Just as "rejected" appears to be operationally quite similar to "aggressive" in the context of this research, we will see that subsequent research will use "proactive-aggressive" in a manner very similar to the category "controversial."

"Social Cognitive Biases and Deficits in Aggressive Boys," also published in 1982, was authored by Dodge and Cynthia Frame. Cited 617 times according to Google Scholar, it combined methodological elements from the studies discussed above and sought to once again to develop the findings of 1980. Dodge uses this as an opportunity to revisit the problem of cue distortion and cue utilization in somewhat different terms. In the first study, he seeks to measure "biased attribution," which is operationally equivalent to cue distortion.[51] Using methods from his 1980 paper, he finds that biased attribution, which can be thought of as a Type I error or false positive, is significantly though not wholly predictive of whether a subject fits into the "aggressive" category. Interestingly, nonaggressive peers also displayed strong attribution biases when the names of aggressive children were used in the experiment, which suggests that "biases" are not the unique marker of aggressive children.

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[51] Although neither is defined rigorously, "cue distortion" and "biased attribution" are quite distinct semantically, as the latter suggests a behavioral tendency and the former suggests a mechanism. One might speculate that attribution biases occur as a result of cue distortion, but this connection is not made experimentally. Presumably, they *can* be dissociated experimentally, since it is not difficult to imagine measurable attribution biases occurring without cue distortion. Insofar as Dodge's actual aggression research is concerned, however, the two categories appear to be interchangeable at the level of experimentation and measurement.

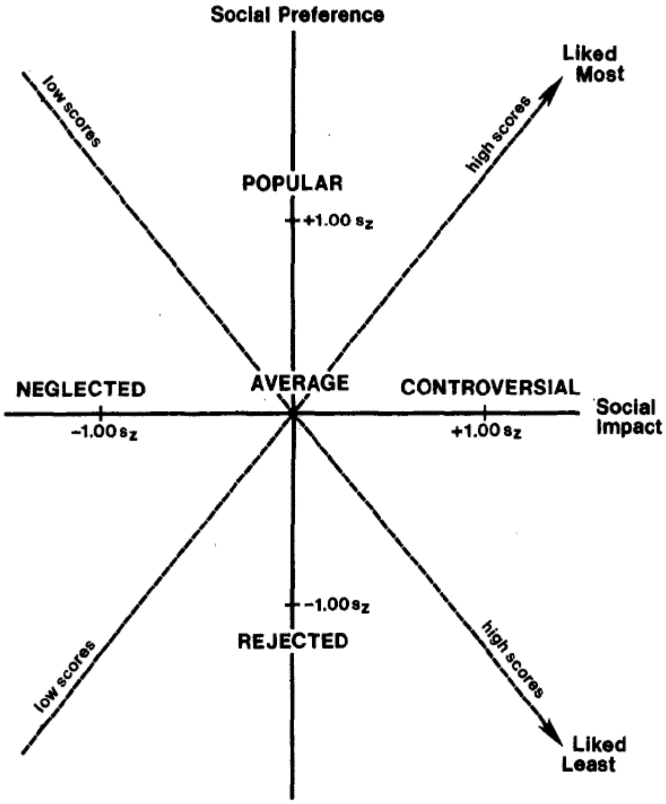

Figure 1, Axes of behavioral coding, from Coie et al (1982).

Because biased attribution fell short of an adequate account of aggressive children by itself, Dodge uses the notion of "selective recall" to supplement his account. In a second study, similar in methodology to those discussed earlier, he found for a second time that aggressive children were less likely to remember information that conflicted with their beliefs. Although Dodge had never measured cue utilization deficiencies clearly in 1980, it seems that this approach is quite similar in what it hopes to describe, at least insofar as it represents a property of higher order cognition as opposed to the perceptual level.[52]

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[52] Perception involves a number of top-down processes, and that it is deeply problematic to characterize it as a functionally distinct aspect of cognition. However, much of the relevant research in the psychology and neuroscience of perception occurred at a later point,

What makes this paper interesting, however, is its third study, which appropriates Coie's methods and applies them to the themes developed in Dodge's earlier papers. In this study, samples of 8 subjects in each condition were chosen for observation in natural settings. The natural setting in this case, was limited to interactions with peers. Behavior was taped, observed, and coded by the research team, with a careful eye for initiations of aggressive behavior. It was found that children in the "aggressive" category (operationally equivalent to the "rejected" category) were more likely to initiate aggressive interactions with peers than were the nonaggressive (popular) subjects. Aggressive children were also substantially more likely to be the targets of peer aggression, which lends more credible support to the notion of a feedback loop than did the original research.

In reviewing their three experiments, Dodge and Frame describe their theory as an "information processing model" for the first time. They use this phrase somewhat off-handedly in their abstract and it does not feature in their discussion, but its appearance is certainly worth noting. Their "model," at this point in the research program, takes the form a paragraph-long explanation of aggression in children. In short, it states that aggressive children carry information in their memories that they are likely to be the target of hostility (perhaps this is where selective recall fits in, although it is unclear), which leads to attribution biases in ambiguous situations. Attribution biases increase the likelihood of aggressive behavior in the subject, which then results in reciprocal aggression on behalf of their peers. This creates a cycle that increases the subject's attribution biases and subsequent aggression to inappropriate extremes. This model assumes that at some point, some children's cognitive representations become distorted, as exhibited by the observed disproportionate rate of initiating peer aggression relative to being its target. The model does not explain why, if the aggression is reciprocal, some children belong in the category "aggressive" and not others, or why some children are likely to be thrown into this cycle to begin with.

Although the model tends to beg the question of why some children are aggressive and others are not, it is not difficult to speculate as to how more extensive explanations may fit into the

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and it should be noted that this aggression research does seem to build from a certain degree of naïve realism with regard to perceptual processes. I will return to this issue at a later point in this chapter.

model. For instance, it may be the case that, as a matter of poor luck, some children suffer more hostile encounters than others. Suppose that 20% of all social encounters between children are hostile, and that each hostile encounter lowers a child's threshold for attributing hostility. If child A lives in a neighborhood with 50 peers and child B lives in a neighborhood with 20 peers, we might expect A to have a substantially greater sensitivity to hostility than B. This interpretation is radically situational in its approach. On the other hand, the model is also compatible with radically innatist approaches. Some children may be genetically predisposed to behave aggressively, and this disposition may become exacerbated by the cycle of hostility. Either interpretation can be seen as deterministic in the sense that they predict that some children will be more vulnerable to cycles of aggression than others, but the latter takes the step of localizing the pathology in a particular group of children in addition to identifying environmental factors. The researchers do not explicitly commit to either interpretation (nor do they need to), although the very category of the "aggressive boy" suggests something of an innatist leaning on their part.

1983 saw the release of a handful of papers in the same vein as those described above. No substantial methodological or theoretical advances appear to have occurred during this period, and information processing had yet to surface as a major theme in the research. For these reasons, I will not discuss their content any further. It is worth noting that of the six papers with which Dodge was involved in 1983, only two have been cited more than 100 times. Of 1982's papers, four of five have received over 100 citations, including one with over 1,000 citations today.

It does not appear as though 1984 was a particularly productive year for Dodge and his colleagues, at least with regard to expanding their aggression research. Dodge wrote a literature review of social development research and collaborated with his University of Indiana colleague Roberta Murphy to produce two papers regarding various aspects of social competence in children. Neither of these papers was widely cited, and they essentially revisited the research questions of previous studies, used very similar methods, and found similar results. For instance, the major finding of one paper is that "the errors by deviant children tended to consist of erroneous labels of prosocial intentions as hostile."[53]

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[53] Dodge, K.A., & Murphy, R.R., & Buchsbaum, K. (1984). The assessment of intention-cue detection skills in children: Implications for developmental psychopathology. Pg. 163.

There is, however, one particularly noteworthy paper from 1984: "Social information processing patterns in child psychiatric populations," which represented a collaboration between Kenneth Dodge and Richard Milich, a hyperactivity researcher from the University of Iowa. In this study, the researchers looked at a sample of children selected by psychiatric professionals. The children suffered from a range of behavioral problems, excluding only retardation and psychosis. Milich and Dodge presented the children with a number of tasks to gauge social cognition, and their procedures did not significantly differ from those described earlier. For instance, cue processing was measured in terms of free recall tasks for testimonies, and attribution biases were observed.

This paper stand out for three reasons in particular. First, it is the first paper in this research tradition to use the phrase "social information processing" in the title, or to devote a substantial part of the discussion to it. Second, it among the first papers to refer to the observed behavioral patterns in abnormal children as or "deficits"—social information processing deficits, to be specific. Third, this is the first paper to propose a rough multi-part model for social information processing.

The model builds from the observations made in Dodge's earlier research. First, Dodge found that aggressive children generate incompetent solutions to situations with ambiguous cues. Second, that aggressive children are more likely to attribute hostility their peers. Third, aggressive children have problems using information to form sound judgments about other people—for instance, we have seen that aggressive children reportedly do not process peer testimonies effectively. Based on these findings, the researchers develop a three-part model that could be applied to the new psychiatric sample. This model asserted that: social information processing deficiencies take place in 1) response decision, 2) hostile attribution, or 3) utilization deficiency. Sure enough, the model correctly predicted that with regard to subjects in the psychiatric sample, measurable deficiencies in at least one of these processes could typically be found.

It is not made entirely clear what his model means. In some places, the researchers simply claim that it captures the three domains of "deficits" observed through the various experiments. In these reading, Dodge and Milich's work is very much in the footsteps of Sternberg and Donders, who built their models out of the layers of experimental procedures. Dodge and Milich seem to have a somewhat stronger theoretical commitment to the 1984 model than Donders or Sternberg had to theirs, however—throughout the discussion it appears as though the researchers hope to generalize the model. Not only do they claim that their experiments capture these particular social information-processing deficits, but they also imply that *every* problem behavior originates from a problem in at least one of these stages of cognition. My understanding is that Dodge and Milich are making a claim at the token level, saying that every behavioral problem is caused by an information processing deficit that occurs in at least one of the above processes. This paper was cited 183 times according to Google Scholar.

In 1984, Dodge was awarded the "Distinguished Scientific Award for Early Career Contribution in Psychopathology" by the American Psychological Association. By 1985, Dodge had advanced to being an Assistant Professor at Indiana University, and then moved to University of Colorado. In 1985 he received the Boyd R. McCandless Young Scientist Award for his research. Despite this honor, 1985 appears to have represented another slow year in terms of publications and experimentation. Dodge and his colleagues produced a small handful of papers that were neither widely cited nor theoretically innovative. The exception to these generalizations is "A Situational Approach to the Assessment of Social Competence in Children," which was later republished in five different anthologies, in 1988, 1994, 1997, 1998, and 1999. This paper likely owes it success, at least in part, to its retreatment of previous themes with greater conceptual clarity than what we see in the jumbled 1980 paper. Although it does not impart any new theoretical insights or experimental methods, it should be recognized as a substantial improvement in terms of research quality.

2.3 The First Watershed: 1986

From the first paper in 1980, one of the major draws to Dodge's work was its appropriation of contemporary findings and theoretical constructs from cognitive science to solve what was fundamentally a problem of social disruption. Vague talk of priming and cue distortion, however, was a poor substitute for a substantial mechanistic account of aggression and related behaviors. Whereas the first six years of the SIP research program were spent laying experimental and theoretical groundwork, the seventh year saw the research solidify into a more ambitious program to identify the pathology of aggression in explicit terms.

Dodge published only four papers in 1986, and only one paper has been widely cited or republished. This was "A social information processing model of social competence in children," in which he appears as the sole author of the paper. This paper introduces the five-step information-processing model, which has already received some mention in my previous discussion. To reiterate the 1986 model, it claimed that social information processing in human children occurs through he following five steps: 1) cue encoding process, 2) representation process, 3) response search process, 4) response decision process, 5) enactment process. A more detailed graphical depiction of this process can be seen in Figure 2. Dodge argues that any social problem affecting a child reflects a problem in at least one of the above processes.

This model is not restricted to aggressive children, although the bulk of the findings used to support the model come from Dodge's own research regarding aggression and the operationally similar neglected children. It is essentially a more elaborate and mechanistic model of the processes described in 1984, as selective recall deficits appear to be a problem in the response search process, cue distortion a problem in the representation process, and cue utilization a problem in either the response decision process or the enactment process.

Unlike the 1984 model, however, the 1986 model does not bear an obvious structural connection to the related experimental design. Whereas each part of the former reflects an experimental measure, the 1986 model does not have a clean one to one ratio of steps to behavioral coding experiments. Rather, it reflects a more computationally inspired approach, and strongly resembles contemporary work in cognitive science for its structure. Given the generality of the five steps, there is no reason why the 1986 model appears to describe *social* information processing in particular—it seems equally applicable to non-social cognition as well. Similarly, it does not explain why an information processing deficit would result in aggression in particular. Moreover, it doesn't convey any special information about why a social information-processing deficit might arise; rather, it provides a very course map of "where" a problem might arise. As I have suggested previously, the five-step model was probably not borne directly from Dodge's experimental work, but appears to have been appropriated from common and structurally similar modeling styles in the field of cognitive psychology.

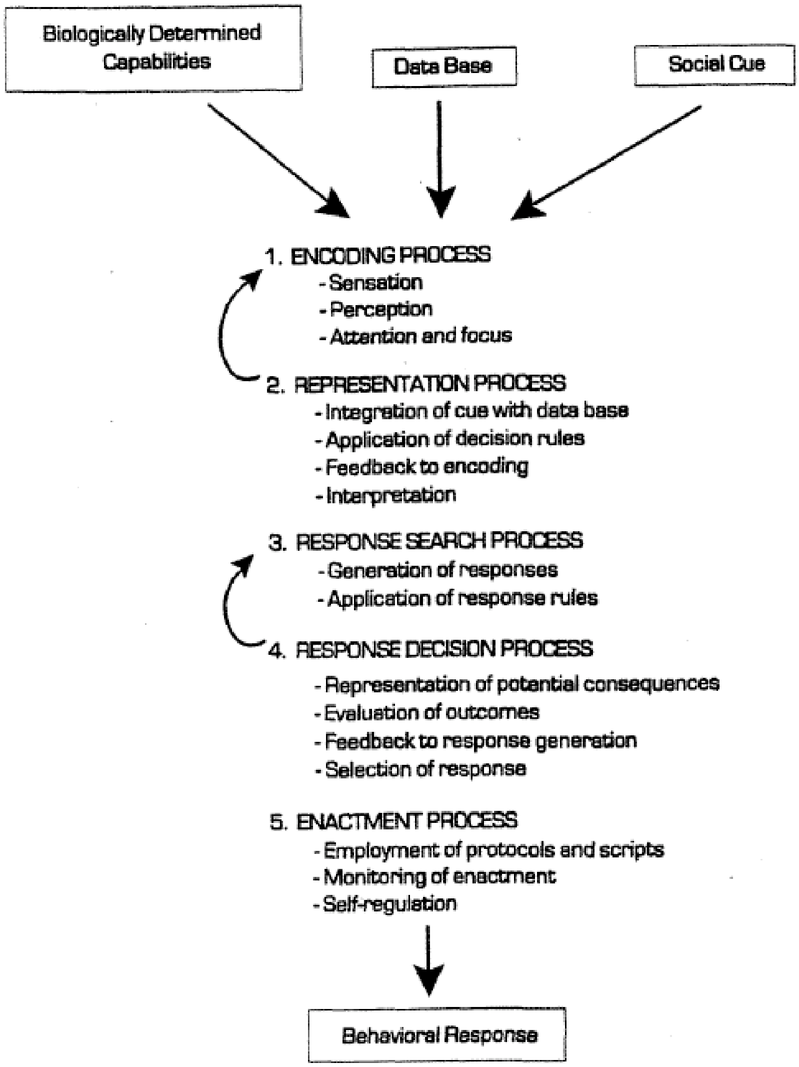
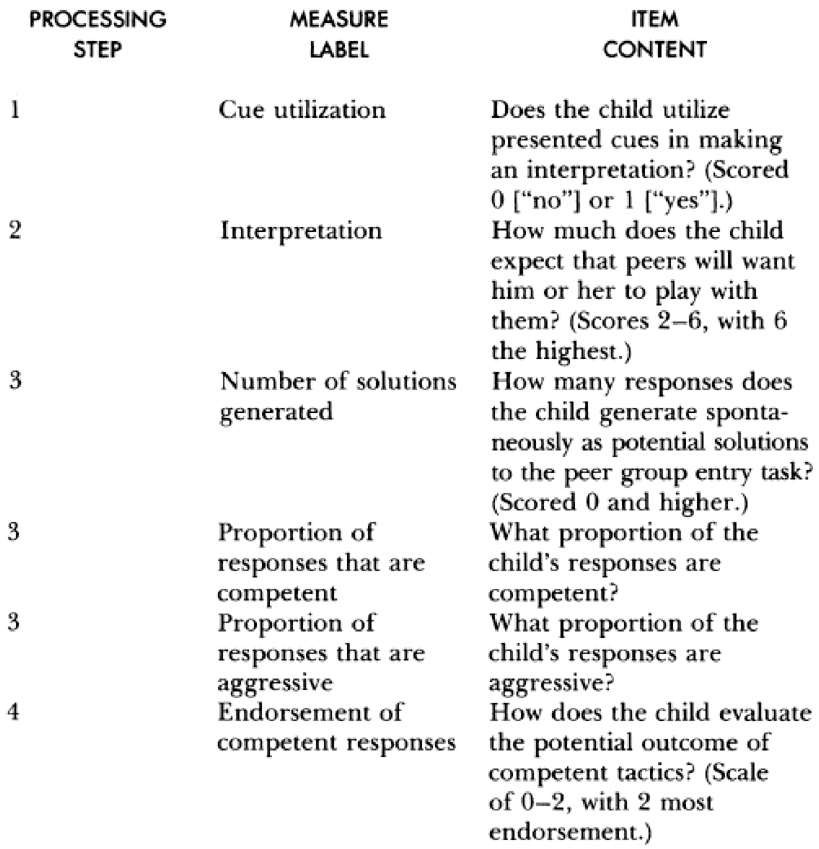

Biologically Determined Capabilities
Data Base
Social Cue
1. ENCODING PROCESS
- Sensation
- Perception
- Attention and focus
2. REPRESENTATION PROCESS
- Integration of cue with data base
- Application of decision rules
- Feedback to encoding
- Interpretation
3. RESPONSE SEARCH PROCESS
- Generation of responses
- Application of response rules
4. RESPONSE DECISION PROCESS
- Representation of potential consequences
- Evaluation of outcomes
- Feedback to response generation
- Selection of response
5. ENACTMENT PROCESS
- Employment of protocols and scripts
- Monitoring of enactment
- Self-regulation
Behavioral Response

Figure 2 Dodge's 1986 model, as represented in Crick & Doge (1994)

Dodge accompanied this paper with a more detailed monograph in which he argued for his model in great detail, "Social Competence in Children." In this paper, he carefully reviews his previous research and relates it to various theoretical movements, both in social psychology and information processing modeling in cognitive science. In this monograph, the authors openly discuss multi-step modeling through the method of mental chronometry, and even cite Sternberg at several points.[54] The authors admit that the five-step model hardly compares to the sophistication of more established information processing models in cognitive science, but they emphasize the advantages of older approaches, most of which fit into the umbrella of drive theory (which I describe in my first chapter).



PROCESSING STEP	MEASURE LABEL	ITEM CONTENT
1	Cue utilization	Does the child utilize presented cues in making an interpretation? (Scored 0 ["no"] or 1 ["yes"].)
2	Interpretation	How much does the child expect that peers will want him or her to play with them? (Scores 2–6, with 6 the highest.)
3	Number of solutions generated	How many responses does the child generate spontaneously as potential solutions to the peer group entry task? (Scored 0 and higher.)
3	Proportion of responses that are competent	What proportion of the child's responses are competent?
3	Proportion of responses that are aggressive	What proportion of the child's responses are aggressive?
4	Endorsement of competent responses	How does the child evaluate the potential outcome of competent tactics? (Scale of 0–2, with 2 most endorsement.)

Figure 3. First part of the coding procedure introduced in 1986, fitted to the steps of the model.

Dodge and his colleagues include a set of studies in this monograph, in which they essentially replicate previous research and use the five-step model to code children's behavior. An example of this can be seen in Figures 3 and 4, which depicts the coding-key for one of the studies. We can see that, although each of the experimental measures corresponds to a single stage in information processing, each stage corresponds to a number of experimental measures. Moreover, some stages are represented far more thoroughly than others in the experimental measure. Step 4 corresponds to seven different experimental measures, whereas Step 1 refers to only one.

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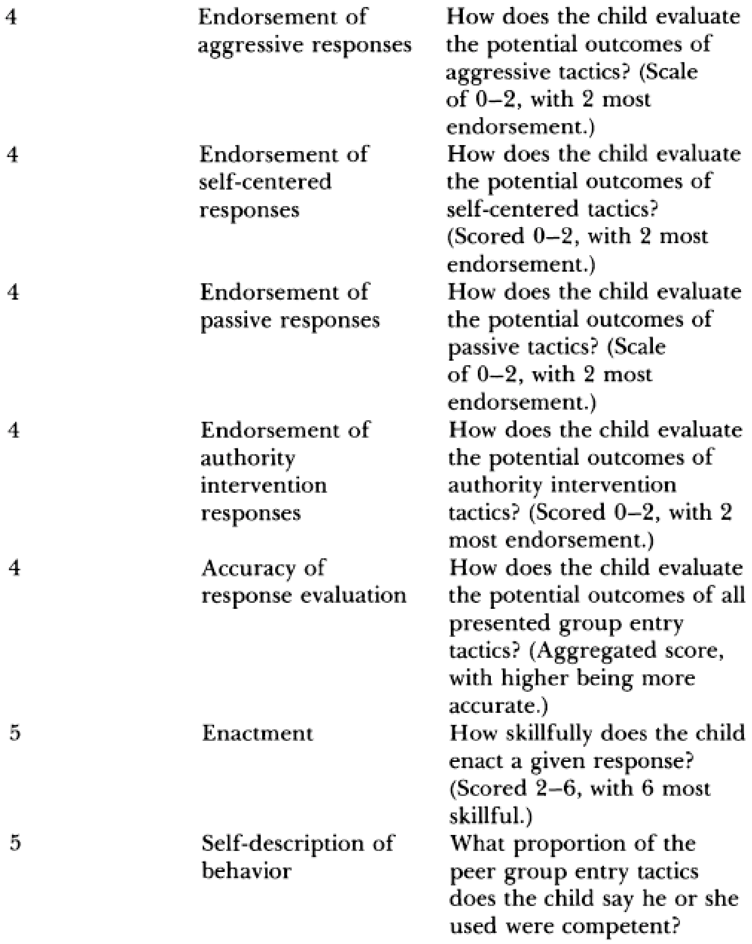
[54] Dodge et al. (1986), pg. 60.



4	Endorsement of aggressive responses	How does the child evaluate the potential outcomes of aggressive tactics? (Scale of 0–2, with 2 most endorsement.)
4	Endorsement of self-centered responses	How does the child evaluate the potential outcomes of self-centered tactics? (Scored 0–2, with 2 most endorsement.)
4	Endorsement of passive responses	How does the child evaluate the potential outcomes of passive tactics? (Scale of 0–2, with 2 most endorsement.)
4	Endorsement of authority intervention responses	How does the child evaluate the potential outcomes of authority intervention tactics? (Scored 0–2, with 2 most endorsement.)
4	Accuracy of response evaluation	How does the child evaluate the potential outcomes of all presented group entry tactics? (Aggregated score, with higher being more accurate.)
5	Enactment	How skillfully does the child enact a given response? (Scored 2–6, with 6 most skillful.)
5	Self-description of behavior	What proportion of the peer group entry tactics does the child say he or she used were competent?

Figure 4. Second half of the coding procedure introduced in 1986, fitted to the steps of the model.

This somewhat contrived matching between the model and the evidence contrasts sharply with the pattern seen in the work of Donders and Sternberg. While Dodge and Sternberg use structurally similar models, it is clear enough that they function in very different ways. Sternberg's model functions as a one to one representation of experimental representations, and does not demand a particularly strong theoretical commitment. Dodge's model, however, does not bear a direct relation to the relevant experimental work, so it cannot be read in the same radically empirical way that one might approach mental chronometry. It does not endow scientists with any new predictive power, which suggests that it should not be read as a purely instrumental device. Some authors portray scientific models as conceptual analogies, as between photons and waves through water,[55] but this does not seem to pertain to Dodge's model. For these reasons, I believe that the most natural reading of Dodge's 1986 model is as a coarse "map" of cognition. It implies a degree of realism about mental processes that it depicts, since they are posited without substantial predictive applications; in portraying "real" processes, it offers something that the world of mental health professionals and researchers had been waiting for since the beginning of the 20th century—a step towards the psychological pathology of social diseases.

The monograph ends with commentary on Dodge's research, provided by the preeminent psychologist John Gottman. Gotman scarcely discusses the content of the research reviewed in the monograph, but goes to great lengths to emphasize that social information processing research plays a vital role in merging the study of social behavior with that of cognition.[56] Clearly, the real value of Dodge's work lay in what it gave the world of social psychology and mental health—a bridge to the far more mechanistic and prestigious realm of cognitive science. Dodge received substantial recognition, both within his field and outside of it. *Esquire* magazine named Dodge in its register of "outstanding men and women under 40," and he transferred from University of Colorado to Vanderbilt University as an associate professor.

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[55] E.g. Hesse (1966).

[56] Dodge et al (1986) pg. 81.

2.4 Growing Up and Out: 1987-1993

Following the introduction of the five-step model in 1986 and Dodge's transfer to Vanderbilt University, his information-processing research program continued to develop and flourish. In particular, 1987 saw a large degree of collaboration between Dodge and other authors, which resulted in a number of extensions of his previous findings.

For instance, Dodge collaborated with Esther Feldman of Miami Medical School's neurology department to produce "Social Information Processing and Sociometric Status: Sex, Age and Situational Effects." In this paper, Feldman and Dodge state that "the purpose of the present investigation was to test the hypothesis that low sociometric status children display biases and deficits in several of the social information-processing steps outlined in [the 1986] model."[57] Samples of various sociometric strata were selected using the same methods as in previous studies, and information-processing deficits were measured by the children's responses to questions concerning various stories. One story involved a child being teased, one involved a situation with ambiguous provocation, and one involved approaching a group of peers. The researchers asked the subjects to answer questions about the story, how the characters should behave, what behaviors would be effective, and so forth. What is interesting is that in this study, the structure of the questions was fit to the five-step model. Panels of observers watched for various behaviors that were thought to indicate problems at specific steps of the model.[58] A panel rated these behaviors. For example, the subject's behavior in evaluating possible courses of action (corresponding to step 4) and their decisions about how a character should behave (corresponding to Step 5), and the subject's behavior was rated on a five point scale: 1=competent, 2=aggressive, 3=passive-ineffective, 4=withdrawal, 5=appeal to authority for an intervention. Unlike earlier studies, this one included samples of girls, which were used to create a contrast to all-male samples.

It was found that "deviant children (rejected and neglected) were found to respond deficiently compared to average and popular children."[59] This result, however, was only found when the subjects listened to stories about children being teased. They also found that older deviant children performed competently, which seems to run contrary to the assumption of feedback loops and vicious circles. A significant amount of variation emerged across gender as well as sociometric status, although no apparent pattern could be identified. I will not discuss the details of these findings, but it suffices to say that the results do not straightforwardly support the stated hypothesis of the paper. The authors interpret these findings as "evidence of the elusiveness and complexity of social information-processing defects among sociometric status children."

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[57] Pg. 212.

[58] E.g. "Why do you think the girls are laughing?" is meant to correspond to the interpretation of cues.

[59] Ibid 211.

In "Hostile Attribution Biases Among Aggressive Boys Are Exacerbated Under conditions of Self," Dodge and Daniel Somberg found that when under stress (induced by paying a child actor to behave in a hostile way), the measured difference between deviant aggressive children and their prosocial peers was significantly increased. A handful of other studies were conducted, most of which explored possible extensions of Dodge's previous work.

The most important paper of 1987 was likely "Social Information Processing Factors in Reactive and Proactive Aggression in Children's Peer Groups," which involved another collaboration with Coie. In this paper, the authors revisit a distinction at the heart of the research program. In 1980, Dodge distinguished between instrumental and defensive aggression, and chose to focus entirely on explaining the latter. In 1987, the term "reactive aggressive" characterized what had previously been known as "defensive aggression." Not only were the methods for choosing samples of reactive and defensive aggressive children identical, but this paper recast work as early as Dodge (1980) in terms of the former label.

While "instrumental aggression" was never operationally defined in Dodge's earlier work,[60] proactive aggression is described in much the same way as in previous studies—as instrumental, deliberate, goal directed and incentivized. Whereas the drive theorists of the previous generation had focused almost entirely on reactive aggression Dodge and Coie link proactive aggression to the type of behavior described in Bandura's attempts to model social learning. Proactive aggression, as will become increasingly clear, also bears a strong resemblance to the earlier category of "controversial," which was used to describe aggressive children with high social impact.

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[60] Other researchers had previously offered a number of operational approaches to roughly these categories. Dodge and Coie do not appear to be drawing from any other authors in particular.

Other authors, such as Huesmann (1998) argue convincingly against conflating instrumental aggression with other similar categories, but Dodge and Coie do not appear particularly concerned with using a precise terminology. This fault is mitigated by the fact that they cite their own work overwhelmingly, and thereby circumvent some of the potential danger of misrepresenting work that had originally been framed in a different vocabulary.

Interestingly, proactive aggression and reactive aggression were not treated as mutually exclusive categories. The researchers selected a sample of rejected children, using the same methods as in Coie's earlier research. The samples were almost entirely black, and all male. Importantly, the control group this time was not a group of "prosocial" children as it had been before in the early 1980s, but a group of children with average likability scores from their peers. Insofar as the experimental control is meant to distinguish deviant groups from the norm, this represents a substantial methodological improvement.

The sample of neglected children was further divided into four groups: 1) reactive aggressive but not proactive aggressive, 2) proactive aggressive but not reactive aggressive, 3) both proactive aggressive and reactive aggressive and 4) non-aggressive. The researchers designed reactive aggression and proactive aggression scales by which teachers could rate each child. The scales consisted of questions to which teachers could respond with a score of one to five, such as "This child uses physical force in order to dominate other kids" as a measure of proactive aggression and "When this child has been teased or threatened, he or she gets angry easily and strikes back," as a measure of reactive aggression. The children were divided along the median scores of the scales into the four categories above. Using these categories, Dodge and Coie repeated familiar experiments to measure hostile attribution biases.

Dodge and Coie were generally concerned with demonstrating that proactive and reactive aggression could be dissociated experimentally, and did not form further hypotheses. To this end, they found relatively weak support for the distinction. While only reactive-aggressive subjects tended to display hostile attribution biases, they also found that these biases were not observed in all of their experiments. They found a positive correlation between attribution biases and reactive aggressive status, but did not find any such correlation with proactive aggressive status. Thus, there appears to be a measurable difference across the groups, but this difference was not always distinct or significant. One study investigated how peers viewed the aggressive children, and it was found that child peers were not able to distinguish reliably between reactive and proactive aggression—essentially, that the experimental constructs did not correlate to how the children tended to view each other's behavior. On the other hand, members of the proactive aggressive groups were more likely to be seen as having a sense of humor and being good leaders, whereas members of the reactive aggressive groups were more likely to be seen by their peers as bothersome. This suggests that there is some reliability[61] to the categories of "proactive" and "reactive," but the distinction is at most a matter of degree, and as of yet there is no unequivocal evidence for a difference in information processing deficiencies across groups. It is also worth considering the possibility that the researchers are not tracking the categories that they intend to, which would no be surprising given the complexity of human behavior. This paper was cited 894 times.

At this point, Dodge's 1986 model had grabbed the attention of researchers outside of his own circle of collaborators. In 1987 alone, Dodge was invited to present his research at eight universities, in addition to seven conferences at which he presented papers.[62] One notable example of SIP theory's growing influence is "Applications of Dodge's social Information Processing Model of Social Competence to the Study of Pro-social Behavior in Children," a conference presentation by David and Louise Perry. These researchers interpret the 1986 model as stating that aggressive children exhibit deficits at *every* stage of processing. It seems implausible that every aggressive child would exhibit a deficit at every stage, since a deficit at a single stage is sufficient to explain inappropriate behavior; a more charitable reading of the Perrys' analysis might be that deficits at every stage have been observed among aggressive children. While the researchers do not make it entirely clear how they approach the model, their discussion is deeply uncritical. They review a hodgepodge of previously un-discussed findings pertaining to social status and misbehavior and argue that Dodge's model does an extremely effective job at capturing these phenomena.[63] They proceed to espouse Dodge's work, and argue that it would be fruitful to generalize its scope to account for prosocial behaviors. Finally, they conclude that the value of Dodge's model is its ability to generate new research questions. With regard to future questions, they allude to the roles of affect and empathy in information processing, as well as the task of accounting for altruism within the same framework as aggression.

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[61] The term "reliable" can be used in a number of distinct senses. I use the term in the statistical sense. Funder (2006) defines this sense of reliability as follows: in measurement, the tendency of an instrument to provide consistent information on repeated occasions, G-11.

[62] This information, as well as much of the biographical information in this chapter, comes from Dodge's CV.

[63] For instance, these researchers apply Dodge's model to findings that popular children are more likely to agree with each other's judgments than unpopular children are likely to agree amongst

By the late 1980s, the number of researchers involved in developing SIP theory had begun to grow substantially. Dodge's doctoral and postdoctoral students at Vanderbilt University had begun to publish their own research. In 1988, Dodge's postdoctoral student Gregory Pettit published his first widely cited paper, "Early Family Experience, Social Problem Solving Patterns, and Children's Social Competence." Pettit would become a frequent contributor to SIP theory during future years. In 1989, Dodge authored 11 papers with 16 coauthors including his post-doctoral student Joseph Price. Price was Dodge's first postdoctoral student to concentrate his research on social information processing and aggression, and published "Reactive and proactive aggression in childhood: Relations to peer status and social context dimensions," which further developed the dissociation between the two forms of aggression by finding a positive correlation between proactive aggression and social status among peers. Nicki Crick, a doctoral student under Dodge, published her own research pertaining to aggressive children in conflict situations. We will see in future research that both Price and Crick become pivotal figures in the development of SIP theory.

During the last two years of the 1980s, Dodge was invited to present his research at 17 different university events, in addition to handful of paper presentations in academic conferences. In 1989, Dodge was awarded a highly prestigious fellowship at the Center for the Advanced Study of the Behavioral Sciences at Stanford, CA. By all appearances, his career was incredibly successful and his research program was being adopted by a number of other researchers. As one often finds in the sciences, many of his coauthors were students working beneath him, and their own work draws heavily from the theory and findings of his previous research.

In 1988, Rowell Huesmann of the University of Illinois at Chicago published a rival model of social information processing. Huesmann had been writing in parallel to Dodge, starting in 1982

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themselves. Many of these applications are quite dissimilar from the experiments devised by Dodge and his colleagues.

with “Information Processing Models of Behavior.” Huesmann does not cite research from Dodge’s research program after 1980, and at no point during this time[64] does Dodge cite Huesmann. For this reason, I assume that their research programs were developed independently for some time, although a later discussion will create room to doubt this. Huesmann’s model draws from the same computational influences as Dodge’s, and shares a similar structure and vocabulary as can be seen in Figures 5 and 6. However, Huesmann (1988) was significantly more concerned with a literal computational analogy than was Dodge (1986), and appears to have relied less on experimental findings. For reasons far beyond my current scope, Huesmann’s work was never as widely cited or applied as Dodge’s, and appears to have undergone far fewer developments over time. In subsequent sections, I will also consider the differences in funding across the two research programs.

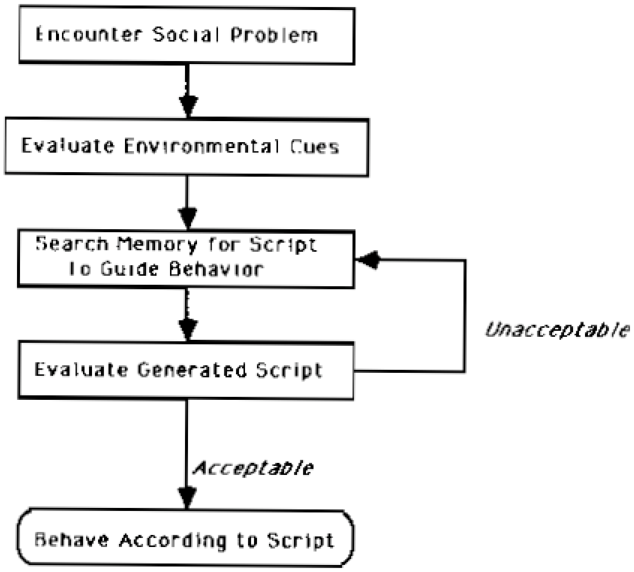
Encounter Social Problem
Evaluate Environmental Cues
Search Memory for Script To Guide Behavior
Evaluate Generated Script
Unacceptable
Acceptable
Behave According to Script

Figure 5. Information processing model from Huesmann (1988).

In 1990, we see a continuing trend in productivity and expansion, as SIP researchers published a number of widely cited papers. In Coie and Dodge’s 1990 paper “Group Behavior and Social Status,” reports that children withdraw from their peers as a function of negative interactions—while perhaps banal, this finding lends support to the notion that behavioral problems involve feedback mechanisms at the level of social interaction. This paper has been cited 599 times.

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[64] That is, at no point in the papers which I have reviewed.

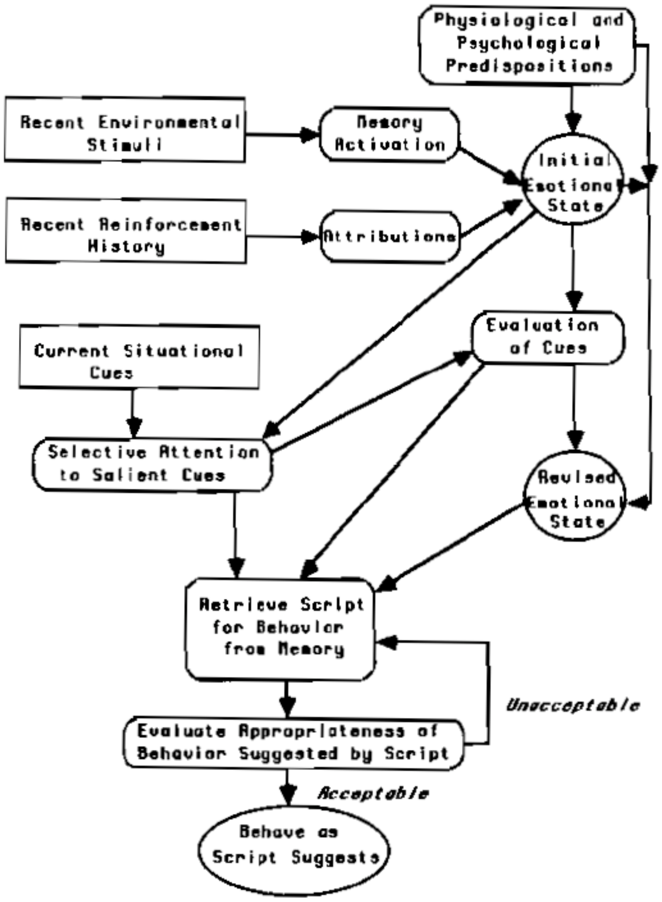
Physiological and Psychological Predispositions
Recent Environmental Stimuli
Memory Activation
Initial Emotional State
Recent Reinforcement History
Attributions
Evaluation of Cues
Current Situational Cues
Selective Attention to Salient Cues
Revised Emotional State
Retrieve Script for Behavior from Memory
Unacceptable
Evaluate Appropriateness of Behavior Suggested by Script
Acceptable
Behave as Script Suggests

Figure 6. Information processing model from Huesmann (1988).

In this year we also see Crick and Dodge produce "The social information processing bases of aggressive behavior in children." This paper provides a thorough review of the previous literature in information processing, with a strong focus on Dodge's findings and 1986 model. It is argued that information processing deficits arising from any particular step of cognition will result in unique manifestations of aggression. Importantly, this is one of the first papers to emphasize the use of SIP theory in treating aggression, as it is claimed that the heterogeneity of components of social information processing results in what one might characterize as a heterogeneous pathology. This is a strong reminder that SIP theory is not merely a cognitive model, but an attempt to contribute to the domain of mental health.

Other papers, such as "Peer status and aggression in boys' groups: Developmental and contextual analyses" typically extended Dodge's methods while staying within the same theoretical and experimental frameworks. It was found, for instance, that proactive behavior strongly correlates with social status among first graders, but that this correlation weakens by third grade. Such studies attempted to repeat previous experiments and take additional measurements so as to gradually and carefully refine the categories under investigation.

In 1990, Dodge, Pettit, and John Bates of the Berry College published "Mechanisms in the Cycle of Violence," in *Science*. This paper has been cited 920 times, and ties Dodge's information processing model into the study of child abuse. The researchers found that physical abuse was a major risk factor for children, even if genetic and environmental factors were identified and accounted for. The authors speculate that memories of abuse create information processing deficits and biases towards attributions of hostility, and make use of Dodge's social information processing framework to supply a mechanism. At this point, the research offers little more than plausible speculation, and it will be many years before this topic is revisited in more rigorous terms.

Social information processing theory continued to both extend outwards and further refine its internal framework in 1991. Dodge and Judy Garber of Vanderbilt linked the 1986 aggression model to the topic of emotion regulation in "Domains of Emotion Regulation," and "Emotion and Social Information Processing." This research does not venture into new experimentation, but instead reviews the literature in the field of emotion-psychology as well as relevant SIP findings (e.g. the effects of "threat" on social information processing, see Dodge & Somberg 1987). As the authors progress in their analysis, they demonstrate that a variety of previously researched topics regarding emotion can be described using Dodge's model. We also see previous topics being revisited in thorough literature reviews, such as "The Structure and Function of Proactive and Reactive Aggression," which focused largely on Dodge's work.

In 1991, we also see the publication of an early third-party literature review by Nameera Akhtar and Jane Bradley in "Social Information Processing Deficits of Aggressive Children: Present Findings and Implications for Social Skills Training." Akhtar and Bradley focus almost entirely on Dodge's research, and particularly on his 1986 model. The authors approach the model one step at a time and scan the vast experimental literature on depression and find examples of information-processing deficits that could be interpreted to occur at that stage. It appears as though they interpret the five-step model as a representation of actual, distinct processing stages.

While almost entirely uncritical of Dodge's theoretical work, Akhtar and Bradley emphasize the danger in using terms as abstract and loaded as "aggression." They discuss the fact that labels such as "aggressive," "maladjusted," "socially rejected," "behavior problem youngsters," tend to be used interchangeably in the literature, although researchers often choose their samples using very different methods. In particular, there is an awareness that "aggressive child" might be a highly heterogeneous group and that different researchers may be measuring different subgroups altogether. Akhtar and Bradley offer a variety of suggestions to shape intervention programs. The different stages described by the 1986 model can be treated separately, they argue. The model emphasizes the complexity of aggression's pathology and highlights the multiplicity of factors, but it also makes it possible to target very specific processes. For instance, they suggest that naturalistic interventions be used to help aggressive children learn to attribute hostility less often. They leave the connection between theory and practice fairly open ended, and it is not clear how SIP theory could actually be implemented in a direct way.

A new branch to grow in 1991 was a connection between aggression and depression research, in "Aggression and depression in children: Comorbidity, specificity, and cognitive processing." This ambitions paper resulted from collaboration between Judy Garber, who had recently approached sad affect from an information processing perspective, Nancy Quiggle, William Panak and Kenneth Dodge. It begins with the notion that both aggression and depression result from specific problems in cognitive processing. Samples of aggressive children were chosen using the same methods as we have already seen. Depressed children were chosen through the standardized Children's Depression Inventory. Subsequently, the two groups were given the tasks from 1986 and measured in the same way. Among the findings was that both aggressive and depressed children are likely to attribute hostility to ambiguous cues, and also report being the recipients of aggression. The experiment did not appear to use a control condition, which makes their results somewhat nebulous. Nevertheless, the two categories displayed only partial comorbidity, and diverged on a number of accounts. For instance, aggressive children were less likely to blame themselves for unsuccessful peer interactions than were the depressed children.

Quiggle et al continued to study aggression and depression, and released "Social information processing in aggressive and depressed children” in 1992. This paper contains a more sophisticated presentation of the themes explored in 1991. Again, aggressive and depressed children were both likely to display hostile attribution biases, and again, aggressive children were more likely than depressed children to respond aggressively to stimuli, while the depressed children were more likely to attribute fault to either themselves or stable external factors. The discussion sections tie Dodge’s work on aggression and social-cognitive modeling into a variety of theories regarding the roots of depression. The details lie beyond my current scope, but it should suffice to highlight the attempt to thoroughly connect Dodge’s SIP model of aggression to other behavioral problems of the mental health community. Dodge’s 1993 paper “Social-cognitive mechanisms in the development of conduct disorder and depression,” offered a thorough literature review that further cemented the notion that aggression and depression could both be accounted for under a single conceptual framework.

Also in 1992, Kenneth Dodge became involved in the Fast Track Project. The Fast Track Project began in 1990 to evaluate the feasibility and efficacy of long-term prevention programs targeting children at risk for conduct disorders. Nine thousand children were screened across four communities, and 891 were deemed to be high risk. The high-risk children were randomly assigned to the Fast Track intervention, and others were kept in control groups. The program involved social skills, anger management, parent training, academic tutoring and home visiting. The Conduct Problems Prevention Research Group, which featured Dodge, published “A Developmental and Clinical Model for the Prevention of Conduct Disorders: the FAST Track Program.” This paper made use of the 1986 model in designing five intervention components to prevent conduct problems, failure in school, and poor social relations.

In 1993, Dodge authored “The Future of Research on the Treatment of Conduct Disorder,” which indicates a continued commitment to the task of applying theoretical work to the “treatment” of undesirable behaviors among children. Dodge recognizes that behavioral problems likely arise form a combination of factors including family, a child’s cognition, peer relations, and “eco-community systems.” Nevertheless, he suggests focusing research an interventions only on a single component. In this paper, he advocates two types of research: 1) developing a technology to successfully change individual [cognitive?] processes and 2) comprehensive measures to prevent conduct disorders.

2.5 A Second Watershed: 1994

Dodge's 1986 model appears to have been a fairly direct appropriation from the computational paradigm that ruled cognitive psychology during the preceding decades. It is not without some irony that in the very same year, James McClelland and David Rumelhart became the bellwethers of connectionism, a growing paradigm in cognitive science that stood in dramatic opposition to more traditional computational models.[65] In two volumes, these researchers published *Parallel Distributed Processing: Explorations in the Microstructure of Cognition*, which offered a new method of modeling cognitive processes across networks of "nodes" that represent populations of neurons. Many researchers interpreted this type of biologically predicated modeling as rendering older sequential models outdated. Despite heated backlash from the older generation of psychologists (and a handful of philosophers),[66] connectionism only grew in popularity through subsequent years.[67]

In 1994, Nicki Crick, then at the University of Illinois at Urbana-Champagne, and Kenneth Dodge published a monumental literature review and revision of the 1986 model: "A Review and Reformulation of Social Information-Processing Mechanisms in Children's Social Adjustment." According to Google Scholar, this paper has been cited 1929 times, which is truly remarkable, as the majority of papers in the SIP research program are cited only a few dozen times. This paper thoroughly reviews the majority of Dodge's papers, building up to and explaining the 1986 model. However, the researchers indicate that contemporary connectionist research threatened sequential modeling, and they enumerate the shortcomings of Dodge's model: A) the apparently rigid sequential structure of the processing model, B) the symbolic paradigm in which the model is rooted is undeniably removed from actual brain processes that have been proposed to occur at a subsymbolic level, C) the model suggests that linear (as opposed to non-linear) algebraic computations occur across processing steps. These features are essentially universal to traditional computational modeling, and if not for the rising connectionist tide, probably would not have seemed like problems at all. Nevertheless, Crick and Dodge were openly responding to the looming threat that the 1986 model would not meet the standards of a new generation.

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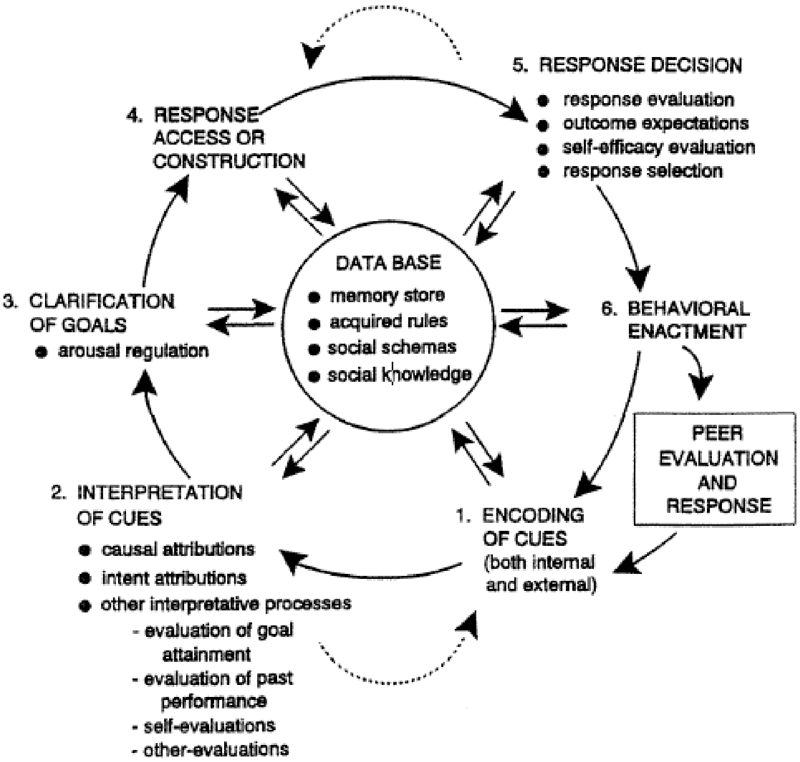
[65] The question of whether connectionism and classical computationalism necessarily conflict has been the topic of much debate. For my purposes, it should suffice to mention that many researchers have assumed the two approaches to be incompatible. Garson (2008) offers a concise summary of the relevant debates and theoretical issues.

[66] Classic examples of this include Jerry Fodor and Zenon Pylyshyn's 1988 paper "Connectionism and Cognitive Architecture: a Critical Analysis" and Steven Pinker and Alan Prince's "On Language and Connectionism: Analysis of a Parallel Distributed Processing Model of Language Acquisition."

[67] Many researchers continue connectionist research today, although its popularity has peaked for a variety of reasons that lie beyond my current scope. Psychologists rarely use classic computational models today (although certain schools of philosophers have yet to abandon them).



5. RESPONSE DECISION
● response evaluation
● outcome expectations
● self-efficacy evaluation
● response selection
4. RESPONSE ACCESS OR CONSTRUCTION
DATA BASE
● memory store
● acquired rules
● social schemas
● social knowledge
3. CLARIFICATION OF GOALS
● arousal regulation
6. BEHAVIORAL ENACTMENT
PEER EVALUATION AND RESPONSE
2. INTERPRETATION OF CUES
● causal attributions
● intent attributions
● other interpretative processes
- evaluation of goal attainment
- evaluation of past performance
- self-evaluations
- other-evaluations
1. ENCODING OF CUES (both internal and external)

Figure 7. Revised information processing model from Crick and Dodge (1994).

Rather than fighting the tide (as many authors of their generation did), Crick and Dodge chose to repackage the 1986 model and evade the above-mentioned problems. The reformulated model can be seen in Figure 7. While it may appear to have erased the sequential stain of its predecessor, it continues to posit functionally discrete steps and it orders them numerically. The new ordering took the following form: 1) encoding of cues, 2) interpretation of cues, 3) clarification of goals, 4) response access or construction, 5) response decision, 6) behavioral enactment. Step 6 feeds back into Step 1, mediated by peer evaluations and responses, and each step connects to a database of stored memories, rules and schemas. The authors go to great lengths to make this depiction of their model compatible with a generally connectionist view.

With regard to the problems mentioned earlier, the new model proposed solutions: A) the new model is cyclical, individuals handle multiple information-processing tasks at once, and they can presumably occur through representations distributed across a network of neurons, B) the new model is presented as modeling a higher level of representation than what one would model with a connectionist approach, and C) processing between steps is probably non-linear, and described by similar spreading activation models, curvilinear and nonlinear functions that other researchers were using to describe neural networks.

Crick and Dodge do not stop their analysis with the formulation of what I will call the 1994 model. Rather, they use their model to address a wide variety of issues in cognitive science that had not previously borne any relation to research in aggression or social psychology more broadly. These topics include: latent mental structures versus on-line processing, speed of processing in automatic versus controlled processing, preemptive processing, cognitive skill acquisition, increases in capacity and speed of processing, increased rigidity of processing patterns, the specific role of emotion in information processing, and so forth. While I cannot justify a detailed discussion of these points, it should suffice to remark that these extensions of the model surpass all previous efforts by far.

The researchers also re-frame many of the observed information processing deficits to some degree. Steps 1 and 2 are inseparable experimentally, and deficits are measured by investigation how children use schemata, attribute causality, and attribute consent. Crick and Dodge cite numerous studies that claim to measure these phenomena, and develop hypotheses such as the following: a) maladjusted children may have memory deficits that do not allow them to store or recall social information adequately, b) maladjusted children may selectively attend to particular types of cues, c) maladjusted children may have well-developed schemata for social interactions that interfere with their ability to process and to use immediate social cues (i.e. the children already have the situation "figured out" and do not process further information).[68] It is worth noting that in addition to using the label "maladjusted," the authors draw most of their examples from experiments involving "aggressive" children. Step 3, the clarification of goals, corresponds to tasks gauging goal-coordination ability; the authors cite research demonstrating that maladjusted children have difficulty coordinating goals in computer games. Step 4, response access or construction, relates to Dodge's previous measures of selective recall. Step 5, response decision, corresponds to response evaluations, stated outcomes of expectations, self-efficacy evaluations, and actual response selections.

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[68] Pg. 83

An additional feature of this paper that makes it noteworthy is that it addresses problems at the methodological level. One of the most serious problems of SIP theory is the weakness of measured differences between conditions, which I described with reference to the first few papers, and which continued throughout the program. Interestingly, Crick and Dodge embrace their chronically weak correlations—given the number of processing stages, they argue that it would be unrealistic to expect measurements of a single processing stage to account for more than a small degree of the variance. This presupposes to some extent that the processes occur discretely, as it were, which suggests that the authors have not fully succumbed to connectionist influences. The authors address a variety of other previously neglected issues, from definitional problems to gender and age as factors in social information processing, although with far less interesting accounts.[69] Finally, the authors defend the sequential presentation of their new "cyclical" model by treating it as a heuristic for interpreting previous findings. This seems to indicate a somewhat weakened commitment to the model as a literal representation of cognition, which is consistent with their assertion that the models functions can occur simultaneously through parallel distributed processing, rather than in a necessarily rigid fashion.

While the 1994 model may offer certain advantages over its 1986 predecessor, the two models share a common weakness: neither provides a compelling account of factors external to a child's cognition. Both models occur within a framework that allegedly involves the cyclical reinforcement of information processing patterns through social interactions, but both models locate the pathology entirely within the child. SIP researchers had made efforts to emphasize the interpersonal nature of aggression and related behavioral problems during the early 1980s, but it seems as if the success and praise of Dodge's 1986 model gave the researchers license to focus their analysis further inward.

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[69] For instance, in discussing gender, the authors hypothesize that non-normative gender behavior is connected to maladjustment. While perhaps indicative of the research's implicit ideological slant, it is only tangentially related to the broader themes in the research program.

This is not to say that the increased attention to internal processes necessarily reveals a change in theoretical commitments. For instance, Dodge's 1992 discussion of the FAST Track indicates that models fulfill the pragmatic function of isolating processes that can be manipulated through treatment. By further developing only a specific component of a large and dynamic model of social interaction and cognition, perhaps the researchers hoped to break the chain of maladjustment with a single well-placed blow to its weakest link. If this is the case, the asymmetry between internal and external components of the model does not necessarily stem from an asymmetry in theoretical commitments.

2.6 Refining Theory and Practice: 1995-2002

Following the 1994 review and reformulation of SIP theory, the researchers continued to maintain a prolific output of paper and conference presentations. During the mid 1990s, we do not see any further substantial developments in the sequential model of information processing, or in the theory surrounding it. As after the 1986 paper, there appears to have been renewed interest in extending the SIP approach to account for a greater variety of behavioral problems.

One of the most important papers from this period was "Social Information Processing Patterns Partially Mediate the Effect of Early Physical Abuse on Later Conduct Problems," which Dodge, Pettit, Bates and Joan Valente, a doctoral student wrote in 1995. This paper stands out for two reasons: 1) the samples of child subjects included girls as well as boys, and 2) this paper explores a new experimental approach. Previous studies generally took roughly the following form: A) identify a group of children as "aggressive" (or otherwise deviant) by using a standardized instrument, B) probe their behavior and look for abnormal tendencies associated with aggression. While admittedly something of a caricature, this depiction does draw out the fact that the researchers construct both the sample-selecting methods and measurement processes with a certain degree of circularity.

Unlike in previous studies, however, the researchers did not construct an instrument to select their sample. Rather, they studied a randomly selected sample of students who had been previously identified as having a history of physical abuse, which was indicated by clinical interviews prior to matriculation into kindergarten. Not only was it found that these children were more likely to behave aggressively by grades 3 and 4, but they also displayed the information processing "deficits" previously discussed, including hostile attribution biases, the inappropriate "access" of aggressive responses, and inappropriately positive assessments of aggression.

Without engaging the theoretical aspects of the SIP research program, its most concrete experimental finding prior to 1995 was that abnormal children display tendencies to behave abnormally, and in particular, that aggressive children display tendencies to behave aggressively. This finding is not trivial—it indicates the reliability of the researcher's operational definition of aggression. However, it does make it difficult to provide a causal explanation of aggression beyond the mechanism of "information processing deficits." The 1995 paper constitutes a first step towards a new direction in experimentation—by breaking the circularity of the study, they were able to anchor social information processing deficits in the more concrete notion of physical abuse, as previously and independently recorded by clinicians. This also highlights the possibility that physical abuse causes the aggressive behaviors under question. Perhaps physically abusive experiences in the domestic environment lead to an increased sensitivity to aggression and hostility that later becomes problematic in educational settings. While the researchers do not commit to abuse being the sole cause of aggression, they speculate that biased social information processing patterns mediate between abuse and later aggression.

In 1995, Dodge was awarded a fellowship at the Center for the Advanced Study of the Behavioral Sciences at Stanford, CA for a second time. The year 1996 saw the first evaluation of the FAST Track program. It was found that the comprehensive program correlated with increased maternal involvement in schools, increased liking ratings from peers, and improved behavioral ratings from teachers. In short, the program appears to lessen "aggression" as the SIP theorists had defined it operationally. As noted previously, the connection between SIP theory and practice in the FAST Track program is rather opaque. Nevertheless, the comprehensive interventions of social workers in the lives and families of at risk children appear to have created a positive impact. The methods used by FAST Track do not differ dramatically from other approaches with substantially different theoretical perspectives. For instance, Raymond Novaco[70] developed similar behavioral interventions, working from a framework of stress and anger management, and focusing on cases of post-traumatic stress disorder in addition to at-risk children.

More so than previous years, the research in 1996 was overwhelmingly directed towards applications of SIP research and the development of behavioral interventions. Dodge and several other authors produced a vast number of papers pertaining to behavioral conduct problems and the FAST Track program, as well as to touching upon race, family structure and gender as relevant factors to the recognition and treatment of behavioral problems. These papers include "Integrating social skill training interventions with parent training and family-focused support to prevent conduct disorder in high risk populations: The FAST Track Multi-Site Demonstration Project," "Physical discipline among African-American and European-

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[70] Interestingly, Novaco held a degree in the philosophy of science, in addition to a PhD in clinical psychology that he earned at the University of Indiana several years before Dodge began teaching there.

American mothers: Links to children's externalizing behaviors," and "Patterns of change in early child aggressive-disruptive behavior: Gender differences in predictors from early coercive and affectionate mother-child interactions." Papers associated directly with the FAST Track program did not refer to SIP theory explicitly, but Dodge was a member of the Conduct Problems Prevention Research Group and played an active role in much of this research.

In addition to these outward developments, SIP theory continued to revisit previous topics with greater clarity. For instance, Crick and Dodge published "Social-information-processing mechanisms in reactive and proactive aggression," which sought to add further validity to the proactive-reactive aggression distinction. In this paper, proactive aggression is framed as interchangeable with instrumental aggression, and the authors review SIP theory with reference to the 1994 model. They formed three hypotheses: 1) reactive-aggressive children would demonstrate hostile attribution biases towards peers' intentions and proactive-aggressive children would not, 2) only proactive-aggressive children would evaluate aggression and its consequences in relatively positive ways, since instrumental aggression anticipates a reward, 3) proactive-aggressive children would select instrumental social goals rather than relational goals more often than nonaggressive children. They based their methods largely in Dodge's previous work, and they found mixed support for each of the three hypotheses. The authors account for recalcitrant results by discussing problems in selecting appropriate samples,[71] and do not alter the SIP framework. This paper also features a detailed discussion of ways in which SIP can inform separate behavioral interventions for proactive and reactive groups, which was notably lacking in the 1987 discussion. It has been cited an impressive 593 times.

Social information processing theory and the associated work in behavioral treatment continued to follow a pattern of outward growth and inward clarification over subsequent years. Between 1997 and 2002, Dodge and numerous other authors published several dozen papers, although there does not seem to have been any substantial change in the content of the research program during this time. The Conduct Problems Prevention Research Group continued to review the FAST Track Program, as in 1999 when it found that gender, race, location and cohort appeared to have little impact on the impact of social workers' interventions. Papers such as "Externalizing Behavior Problems and Discipline Revisited: Nonlinear Effects and Variation by Culture, Context and Gender," worked within the conceptual and experimental framework of earlier work to clarify possible causal relations between harsh parental discipline and aggression. In 1998, Rowell Huesmann wrote a thorough literature review of Social Information Processing models in which he retrospectively compared his own models to those of Dodge and his colleagues. While their research may have been independent at one point, Huesmann certainly came to know his counterpart's work quite well. This was Huesmann's last publication on the topic of Social Information Processing to date.

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[71] Samples were chosen and tested with effectively the same methods as Dodge and Coie had done in 1987. The most significant recalcitrant observation was that the reactive group was not abnormally likely to make hostile attributions, which contrasts with the findings in 1987. One of the sampling issues raised was the use of an African-American male sample in the 1987 a multiracial sample with both genders in 1996.

As SIP grew in popularity, Dodge achieved ever-greater recognition. After gaining tenure as the Alexander Heard Distinguished Service Professor at Vanderbilt University in 1997, he transferred back to Duke University, where he has remained until the present day as a William McDougal Professor of Public Policy as well as a professor of psychology and neuroscience. In 1999, Dodge was named one of the 2,000 Outstanding Scientists of the 20th Century by the International Biographic Center of Cambridge. Also in 1999, Dodge became the first director of the Center for Child and Family Policy at Duke University. By 2003, Dodge had been honored as a distinguished visiting scholar or lecturer at 13 different universities, from the Chinese University of Hong Kong to the Universita di Roma "La Sapienza" in 2000 and 2001, respectively. In parallel to these professional developments, the FAST Track program absorbed $60,000,000 in federal funding, making it the single largest recipient of federal support in the history of aggression research.[72]

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[72] See Duke's "Office Hours with Kenneth Dodge" interview, filmed March 2010.

2.7 Stepping Away From the Lamppost: 2003-2010

2.7.1 Framing Recent Research

Insight often resides in humor, and it may benefit the ensuing discussion to begin with a familiar parable: Returning from a soiree late one dark night, I noticed my friend Todd scouring the pavement on his hands and knees under the light of a lamppost. "I'm looking for my key," he said. I joined the search, but noticed that Todd seemed to be going in circles, revisiting the same spot every few minutes. Sensing that something was amiss, I asked where he had lost it. "Well," he replied, "I'm fairly certain that I dropped it a few hundred meters down Mayfield Road, but the light's best over here."

We have seen a perennial asymmetry in the development of SIP theory: researchers routinely claim in their discussion sections that aggression emerges through dynamic processes at the level of social interaction, yet the various measurements and models that they use generally focus on the behavior of individuals. This generalization is not without exceptions, but it does appear as though researchers in the wake of the popular 1986 model largely abandoned the study of interpersonal dynamics[73] in favor of information processing at the cognitive level. To some extent, this reflects an assumption that targeting a single well-understood component could break the cycle of aggression. However, this raises the further question of why the researchers would focus on the behavioral and cognitive patterns of aggressive children in particular, and to the general exclusion of other relevant factors.[74]

The reason, I believe, is that it is far more difficult to reliably measure complex interpersonal behavior over large time windows than to measure differences in how individuals respond to standardized tasks at any given time.[75] Much like Todd under the lamppost, I believe that Dodge and his colleagues were searching where they could take measurements, and not necessarily where they expected their answer to ultimately lay.[76]

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[73] This topic had received substantially more attention in earlier papers, as we see in the informal model presented in 1982.

[74] E.g., given the known importance of observer biases in attributions of aggression, the researchers could have focused their research on "hostile attribution biases" among "non-aggressive" peers with regard to the "aggressive"" children. Some of the early studies demonstrated that non-aggressive children did in fact display strong attribution biases towards "aggressive" children, but the researchers did not explore this phenomenon further.

We will see that within recent years, the SIP research program has grown bolder in its approach to both measurement and modeling, and I would like to extend the analogy of the lamppost one step further—the researchers have begun to explore the periphery of the visible pavement and let their eyes accustom to the darkness. This shift did not occur discretely at any particular point, but implicitly and over the course of many years. Moreover, this trend has continued up to the present day, and one might expect it to continue its trajectory in future years as well.

2.7.2. The Aggressive Boy Begins to Fade

Published in 2003, "Peer Rejection and Social Information-Processing Factors in the Development of Aggressive Behavior Problems in Children" exemplifies the shifts in SIP theory alluded to in the previous section. This paper was authored together by Dodge, Jennifer Lansford, Pettit, Fontaine and Price. Pettit, Fontaine and Price had all worked with Dodge as postdoctoral or graduate students during the 1980s and 1990s, and had all become established researchers by this time. Lansford had studied at Duke as an undergraduate during the 1990s, and returned to work for the Center for Child and Family Policy after completing her PhD at the University of Michigan. While I will be discussing a methodological break between this paper and its predecessors, one should keep in mind the continuity within the group of researchers. That is, this paper was written by the same authors as much of the important SIP work of previous decades.

The title of this paper is itself part of what makes it noteworthy. While it may appear unremarkable, the phrase

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[75] Recall that observer agreement dropped from 100% to 68% in an early study of interpersonal behavior.

[76] In framing this point, I draw Helen Longino's discussion in "What do we measure when we measure aggression?" In particular, Longino alludes to the same parable in describing the relationship between measurements of individual behavior and the study of social problems such as crime rates.

"aggressive behavior" is somewhat unusual in that it does not reify the category of the "aggressive boy." Only small handful of SIP papers used "aggressive" in their titles as a predicate to describe "behavior" prior to about 2000; during the 1980s, only about 20% of SIP papers followed this pattern. More common were papers that used the word "aggression" as a noun, as well as those that used "aggressive" as a predicate to describe "boys" or "children." Previous studies had generally investigated the "aggressive boy," or perhaps aggression within certain children who had been labeled as aggressive by their peers and teachers. Even studies that looked at maladaption more broadly (and investigated categories such as "neglected" and "controversial") continued to reify and reinforce the category of "aggressive boy," as we see in Coie's research from the early 1980s. The most noteworthy exception to this trend is the 1995 paper "Social Information Processing Patterns Partially Mediate the Effect of Early Physical Abuse on Later Conduct Problems," which used randomly selected samples and was not framed as a study of aggression at all. Starting in 2003, "behavior" began to follow "aggressive" in an increasing proportion of SIP paper titles, which suggests a general change in focus.

The authors articulate four research questions: 1) whether knowledge of early social rejection incrementally predicts subsequent aggressive behavior, 2) whether social rejection can exacerbate preexisting behavioral problems, 3) whether the effects of social rejection vary across genders, 4) which mechanisms might moderate the interplay between rejection and aggression. Rejected children were chosen based on peer and teacher reports, using methods that were nearly identical to those used to find "aggressive" children previously. Effectively, the rejected child is one who is particularly disliked by their peers, and who displays antisocial behavior, including aggression. The methods for measuring aggression and information processing deficits were appropriated from earlier studies.

Methodologically, this study was nothing new. However, the presentation and interpretation of information is quite different. While the authors do cite their previous work frequently, two features appear: 1) the authors cite a wider variety of sources in supporting their assumptions than one sees earlier and 2) the authors make absolutely no mention of previous SIP models, although they do cite the papers in which they appear. Because the 2003 experiments were extremely similar to those discussed previously, the reason for these changes in citation could not stem from changing methods. Citations are the memory of the sciences, and here we see an act of selective recall that constitutes an effort to systematically abandon older ideas.

Perhaps we owe this explanatory change to Occam's Razor—why should a researcher posit an elaborate model of cognition if most of the model's components play no explanatory role? Perhaps the authors had become sensitive to the danger in reifying an unobserved sequence of cognitive steps, given only indirect behavioral data. On the other hand, perhaps the researchers simply did not think that it was necessary to discuss their models yet again, as they had been firmly established by previous papers.

The move towards somewhat more empirical, a-theoretical analysis was accompanied by a new emphasis on the complexity of behavioral problems in humans. In 2004, William Arsenio of Yeshiva University and Elizabeth Lemerise of Western Kentucky sought to further specify mechanisms of SIP theory within a framework of models developed undependably by moral development researchers. Dodge and David Rabiner, a Duke colleague, responded with their paper "Returning to Roots: On Social Information Processing and Moral Development." They acknowledge that Arsenio and Lemerise had identified potentially insightful issues, but resisted any commitment to moral development models. Moreover, they insisted that the study of SIP investigate additional concepts pertaining to the nature of latent cognitive processes, including "working models, schemas, scripts, object relations, classical conditioning." They even end the paper with a lengthily discussion of the methodological challenges in integrating theories of latent mental processing into information processing research. An air of suspicion towards the modeling of latent mental states permeates this paper, and stands at odds with the highly speculative cognitive models developed in 1986 and 1994. [77]

This emphasis on the complexity of behavioral problems was not limited to the above paper. Also in 2004, Dodge published "The Nature-Nurture Debate in Public Policy." In this paper, Dodge argues against the often-presumed separability of genetic and environmental factors in child development. The picture of child development that emerges fro his discussion is dynamic and nuanced; genetic and environmental factors interact through largely unknown mechanisms and account for the tremendous variance observed in human behavior. Dodge proceeds to outline possible reforms in child education, and stresses the notion of flexible curricula that could be customized to fit individual children optimally. In this year alone, Dodge authored 23 papers, many of which sought to clarify the interplay of environmental factors in child development, as we see in "Ethnic differences in the link between physical discipline and later adolescent externalizing behaviors" and "Parent academic involvement as related to school behavior, achievement, and aspirations: Demographic variations across adolescence."

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[77] Interestingly, the literature paper gave a new, retrospective account of the roots of Dodge's early work. While Dodge (1980) situated his work in the context of contemporary drive theorists and social skills theorists, Dodge (2004) tells a somewhat different story. Dodge (2004) describes his 1980 work as an outgrowth of Chandler (1973), who attempted to apply Piaget's theory of perspective taking in young children to the domain of social interactions. Piaget (1965) had developed a rough model of how children learn to take the spatial perspectives of peers (i.e. imagine how someone else in the room might see a picture tilted away from them). Chandler had hoped to show that aggressive children had difficulty taking the perspectives of others.

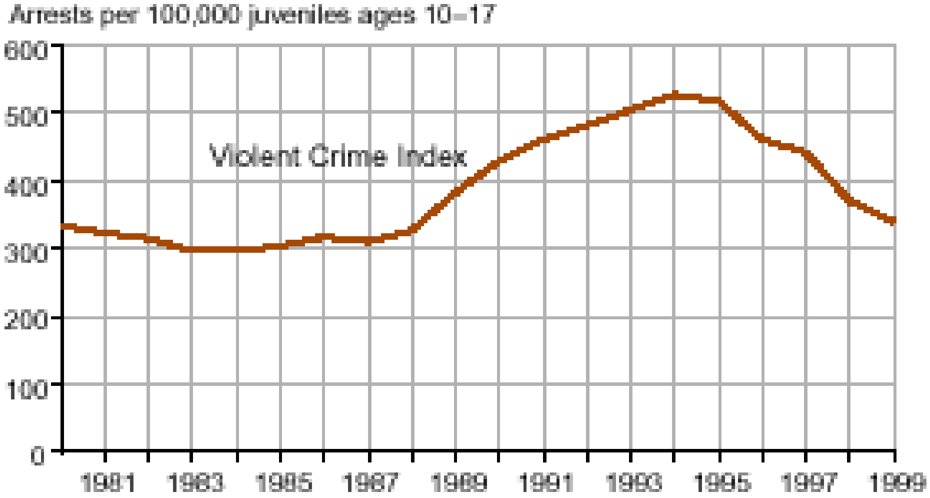

Figure 8, Juvenile arrest rates, 1980-1999, from 2001 National Report Series Bulletin.

While I cannot account for why this shift away from theoretical explanations for the "aggressive child" occurs during the 2000s, one might entertain a number of possibilities. One is that the researchers became aware of the dangers of reifying an abstract category, and made a deliberate shift. Another is that the change reflects larger demographic shifts. As Figure 8 demonstrates, the arrest rate among juveniles had decreased substantially over the late 1990s. Some researchers, including the prolific economist Steven Levitt (2001), have linked this drop in juvenile crime to the legalization of abortion during the early 1970s. This would suggest that the set of "aggressive boys" may have actually grown smaller over time, and consequently so did the need for an explanation. This would have left the SIP research program with a shrinking demographic in need of understanding, and could have encouraged a shift in explanatory focus. Still another explanation is that the shift has deep roots, and simply reflects a stage in a longer historical process. For instance, the 1994 model seems to demand fewer theoretical commitments than the 1986 model, and perhaps the research has been drifting towards empiricism all along.

2.7.3. A New Type of Modeling: A Third Watershed?

In 2006, we see a renewed interest in modeling SIP in Dodge and Fontaine's paper "Real Time Decision Making and Aggressive Behavior in Youth: a Heuristic Model of Response Evaluation and Decision (RED)." In this paper, the authors do not engage with empirical data, but instead develop a new model to aid subsequent research. Aside from presenting previous SIP research to establish theoretical motivation, the researchers review the work of numerous other contemporary aggression researchers, including Heusmann. The authors emphasize the divide between models of aggression that emphasize the rewards of aggression (i.e. approaches that focus instrumental or proactive aggression) and those that treat aggression as an impulsive, reactive behavior. For the first time, Dodge and Fontaine attempted to account for both types of aggressive behavior with a single model. In the words of the authors, this model is meant to "incorporate both impulses and rational thought."

Despite the authors' extensive involvement in previous SIP modeling, the 2006 model bears little resemblance to its predecessors. A graphic displaying the structure of the model can be seen in figure 9. Like previous models, it contains sequential elements, but unlike some previous models, it does not appear to have been built by recasting the composition of an older model. Rather, this model focuses entirely on "response decision," which was a component of the 1994 model. While it nominally represents a component of its predecessor, it leaves little explanatory power for the other components and can easily stand by itself as a heuristic for studying the range of behavioral problems that SIP theory is concerned with.

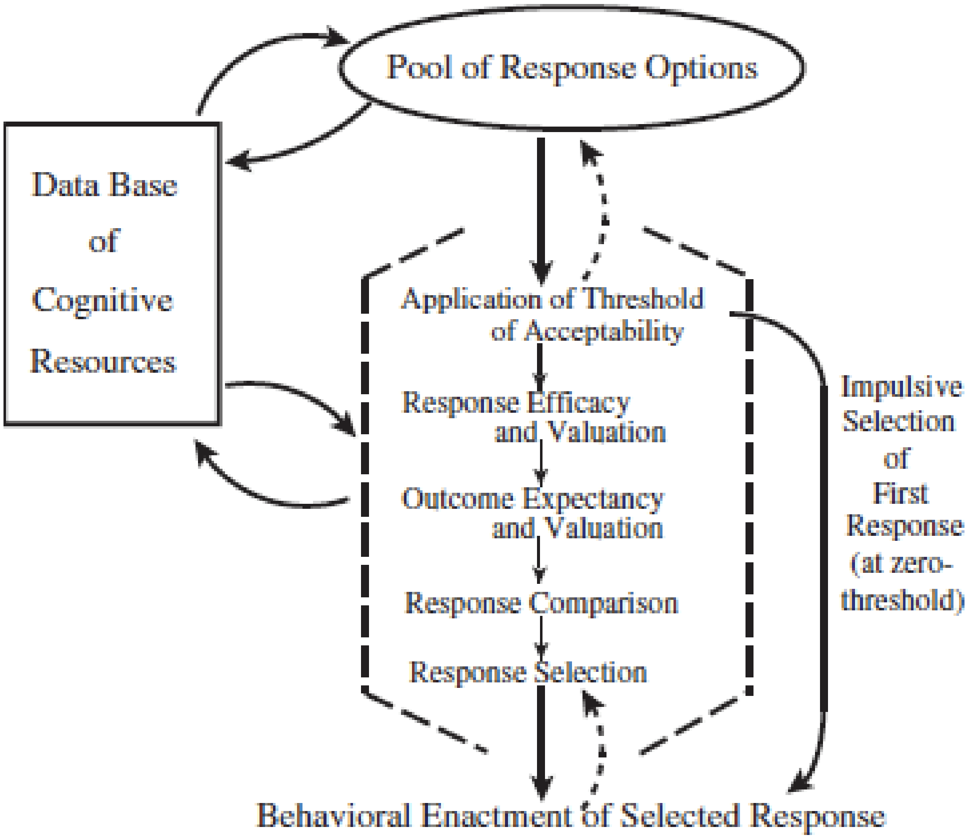
Pool of Response Options
Data Base of Cognitive Resources
Application of Threshold of Acceptability
Response Efficacy and Valuation
Outcome Expectancy and Valuation
Response Comparison
Response Selection
Impulsive Selection of First Response (at zero-threshold)
Behavioral Enactment of Selected Response

Figure 9. 2006 RED Model from Fontaine's research.

A number of important factors distinguish the 2006 model form its predecessors. One factor is that it makes an explicit commitment to modeling "real-time" cognition. That is, it attempts to model cognitive processes that actively occur when a person makes a decision, as opposed to a set of latent processes that may not play an active role (e.g. a database of memories). Previous models were ambiguous as to this commitment.

Another factor that sets the 2006 model apart is that the authors make no ontological commitment to the processing stages represented in the model, and does not require that they actually occur in the brain during cognition. Within the space of their abstract, they remark, "these components are a heuristic to describe decision making, though it is doubtful that individuals always mentally complete these steps." Both the 1986 model and the 1994 model were presented as depictions of actual processing steps, although not necessarily discrete steps in the latter case. The 2006 model, on the other hand, is intended as a theoretically non-committal "heuristic"—

a distinctly instrumentalist turn for the researchers.

Yet another important difference separating the 2006 model is that it was used as the basis for mathematical modeling. Dodge and Fontaine demonstrate that its stages may be represented mathematically, and proceed to describe possible computations that could occur within particular processing stages. For instance, they focus on the evaluation of the "strength" of the cognitive representation of a response option. These mathematical models are of no empirical value at this point in the research, but indicate an effort to obtain more precise predictions from SIP models. None of the previous models were particularly predictive in nature, and aside from the task of dissociating processing stages form one another experimentally, they were of little relevance to experimental procedures. This 2006 model, however, represents a first step towards quantitative predictions of behavior (and potentially neural activity).

Spearheaded by Fontaine, research into RED modeling continues to this day. The researchers have yet to develop the mathematical models into predictive instruments, but efforts have been made to connect the 2006 model to the experimental literature. In 2008, Fontaine published "On-line Social Decision Making and Antisocial Behavior: Some Essential but Neglected Issues," in which he offers a throughout literature review of numerous issues in cognitive science and aggression research as they relate to SIP theory, and the RED model in particular.[78]

In 2008, Fontaine, Chongming Yang, Dodge, Bates and Pettit collaborated to publish "Testing an Individual Systems Model of Response Evaluation and Decision (RED) and Antisocial Behavior Across Adolescence." This paper uses a longitudinal study to measure the reliability of RED task performance as an indicator of behavioral problems. RED assessments were taken with the procedures previously used to measure processing deficits in response decision, and consisted in coding subjects' responses to a series of vignettes meant to simulate realistic social situations. The authors champion the "individual systems" hypothesis, which states that an individual's social cognition is the proximate cause of behavior. This fits within a larger debate regarding the proper level of analysis, which Fontaine

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[78] Fontaine begins his discussion with the sentence "The last quarter century has witnessed considerable progress in the scientific study of social information processing (SIP) and aggressive behavior in children." My third chapter will focus largely on the evaluation of this claim.

(2005) reviewed thoroughly. The details of the 2008 study lie beyond the present scope, but it is worth noting that this was the first paper to incorporate RED into experimental practice. It is also worth noting that the researchers did not select the subjects on the basis of their behavior or popularity ratings; rather, the researchers investigated an entire cohort of students systematically over a period of several years. This sample had already been the subject of childhood studies, and this study followed the subjects through their early adolescence.

Another important paper was "Development of Response Evaluation and Decision (RED) and Antisocial Behavior in Children and Adolescence," authored by the same group of researchers in 2009. This paper continued the longitudinal study of the abovementioned adolescent population as well as a child population, and looked at whether measures of RED processes were stable over time or predictive of conduct problems. The researchers used a cross-lagging model to represent RED scores to behavior, and a sample of this modeling tool can be seen in figure 10. It was found that RED scores were internally consistent over time, but were poor predictors of actual conduct problems among children. Among adolescents, however, aggressive RED scores were strongly associated with antisocial behaviors. While the 2006 model was not useful as a predictive instrument in this study, the study did succeed in demonstrating the reliability of RED measurements.

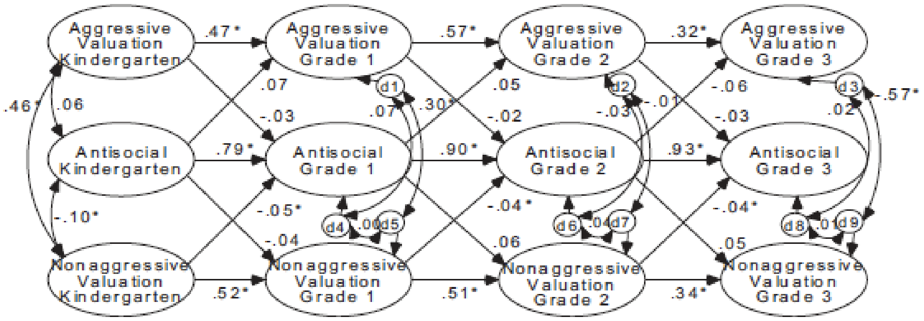
Aggressive Valuation Kindergarten
Aggressive Valuation Grade 1
Aggressive Valuation Grade 2
Aggressive Valuation Grade 3
Antisocial Kindergarten
Antisocial Grade 1
Antisocial Grade 2
Antisocial Grade 3
Nonaggressive Valuation Kindergarten
Nonaggressive Valuation Grade 1
Nonaggressive Valuation Grade 2
Nonaggressive Valuation Grade 3
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Figure 10. Example of the analysis of longitudinal studies. Taken from Fontaine et al (2009).

"Social Information Processing and Aggressive Behavior: a Transactional Perspective" was another 2009 paper by Fontaine and Dodge. It situated the RED research in the framework of transactional modeling, which treats human development as the product of bi-directional influences of the individual and its environment. This approach was developed by Arnold Sameroff (2000, 2003) of the University of Michigan, and offers a more explicit depiction of the feedback loops posited by early SIP theorists. While the 2006 RED model accounts for a very specific set of cognitive processes, the researchers depict it as a single component of a dynamic and causally interconnected system. RED is treated as a factor that directly influences environmental variables, and therefore plays a central, if poorly understood roll in development. Rather than using information processing deficits as an explanation for deviant behavior as previous papers had done, this approach uses the cognitive model to highlight the unanswered questions in SIP research, and open new research topics for future researchers to investigate.

2.7.4 Branching Out

In addition to the development of RED modeling, recent years have seen an explosion of new research topics connected to SIP theory. I will not discuss these in any detail, but will list only a handful of papers since 2003 that illustrate this point. In 2008, Sarah King, Daniel Washbursch, William Pelham, Bradley Frankland, Brendan Andrade, Sophie Jacques and Penny Corkum collaborated to publish "Social Information Processing in Elementary-School Aged Children with ADHD: Medication Effects and Comparisons with Typical Children. In 2007, Friedrich Losel of Cambridge, Thomas Bliesener and Doris Bender of the universities of Kiel and Erlargen-Nuremberg resepectively, published "Social Information Processing, Experiences of Aggression in Social Contexts, and Aggressive Behavior in Adolescents." Dodge, Pettit, Fontaine, Lansford and Bates also worked with Joseph Crozier and Robert Leveonson to investigate heart rate activity as an indicator of deviant SIP in the 2008 paper "Social Information Processing and Cardiac Predictors of Adolescent Antisocial Behavior." Researchers also extended SIP to romantic relationships, as we see in Fite's 2008 paper "Social Information Processing Mediates the Intergenerational Transmission of aggressiveness in Romantic Relationships." We also see more explicit discussions of SIP in naturalistic settings, such as Erath's 2009 paper "Who Dislikes Whom, and For Whom Does it Matter?"

2.8. Letting go of the "Aggressive Boy"

Earlier in this chapter, I discussed the weakening concept of the aggressive child in the SIP literature. Since the early 2000s, this trend continued, and became far more dramatic within the last few years. We see a dramatic example of this in the recent work of Joseph Price, Dodge's student during the late 1980s. Price had been researching behavioral issues among "maltreated" children since the 1990s, and while his work borrowed heavily from Dodge's methodologies, he did not generally focus his independent research on developing SIP.

In 2003, Price published "Hostile Attributional Tendencies in Maltreated Children," which revisited a familiar topic from Dodge's early work. Rather than selecting samples through the use of popularity ratings or behavior, Price chose children who had previously been identified by country social workers as legally maltreated. These children had been removed from their homes at least once, and the forms of maltreatment ranged from inadequate physical and health care, unhealthy living conditions, physical injuries, threats, and witnessing violence between parents. Using procedures similar to those seen throughout Dodge's research, Price measured hostile attribution biases. Price found that relative to a non-maltreated control, physically abused boys were particularly likely to display hostile attribution biases towards peers, parents and teachers. Furthermore, Price found a positive correlation between the frequency of abuse and the likelihood of attributing hostility. While this research does indicate that perhaps the category of the "aggressive boy" might reflect a social reality, the study did not begin by presupposing the existence of such a category. The researchers attempt to actually locate cause of hostile attribution biases experimentally, which contrasts with common practice in SIP research of attributing behavioral tendencies to "information processing deficits." While physical abuse and processing deficits do not preclude one another as explanations, the latter begs the question of causality, and they should be distinguished on this ground.

Price has continued to contribute to SIP theory through the use of samples of legally maltreated children, such as the 2008 paper "Social Information Processing Patterns of Maltreated Children in Two Social Domains." However, Price has also extended his analysis to explore other possible causes of behavioral problems. For instance, in 2009, Price worked with Christie McGee, Olivia Bjorkquist, Sarah Mattson and Edward Riley to publish "Social Information Processing Skills in Children with Histories of Heavy Prenatal Alcohol Exposure." This research deals with developmental problems that are considerably more far-reaching than mere juvenile delinquency, which makes it difficult to compare it to most of the SIP literature. Nevertheless, it deserves special note for its inquiry into the causal factors underlying the behavioral feedback loops and transactional processes that presumably occur over time. The aggressive boy has nearly disappeared in this research, and has been replaced with a far less value-laden image of a child developing under the influence of quantifiable and measurable, if intertwined, causal forces.

This approach uses the predicate "aggressive" to describe behavior rather than children, and this fundamentally alters the structure of the pertinent experimental reasoning. Throughout much of its development thus far, SIP theorists have treated "aggression" as the independent variable upon which other behavioral measures could be mapped. At this point in SIP theory, however, we see that the theorists treat aggression as a dependent variable, which they plot against far more concrete categories, such as documented histories of physical abuse or maternal alcohol intake during pregnancy.

2.9. A Graphical Portrait of SIP Theory

History can be seen as the selection of particular strands of information to be remembered from among a vast set of otherwise unknown facts. My own methodology of close reading makes the process of selectivity all the more pressing, as I face the constraints of my own reading speed, the limited availability of relevant documents, and the sheer volume of material. Despite my best efforts to provide an accurate narrative, my discussion displays a strong bias towards documents authored by prominent researchers or published in major journals, and a tremendous amount of pertinent information simply could not be included. For this reason, I would like to provide what Franco Moretti (2005) refers to as a "distant reading." This approach contrasts with the practice of close reading, and consists in the use of graphs, models and statistics to approach sets of texts. In the present case, this would involve stepping away from the masonry of SIP articles in order to admire the architecture of the research program at a level of considerable abstraction.

First, I would like to provide a graphic illustration of Dodge's research career. Figure 11 shows the growth of the collection of SIP papers authored by Dodge, whose curriculum vitae provided a basis for tabulation. The estimate is conservative, and includes only those journal articles whose titles pertain directly to social information processing or relevant behavioral conduct problems. It does not include conference papers or presentations or papers in which Dodge was not listed as an author.

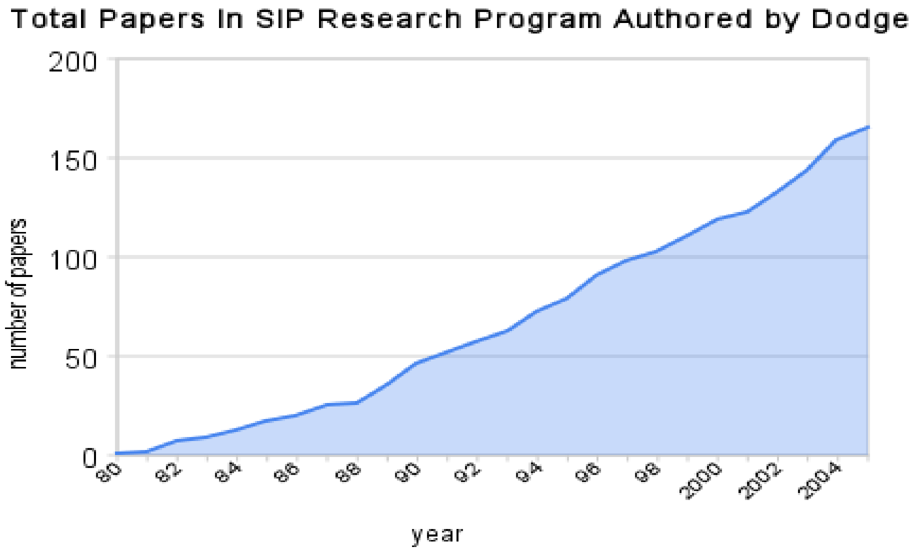

Figure 11 Dodge's authorship on aggression and deviance, as indicated by his CV

Second, I would like to look at the spread of growth of SIP research as new researchers became involved. Figure 12 displays the proportion of papers on which Dodge was listed as the primary author as a subset of the total number of papers that he authored each year. Additionally, this graph displays lines of best fit for both papers in which Dodge was listed as the primary author, as well as the total number of his papers per year. We can see that the slope of the former is quite low, and Dodge's primary authorship is quite stable over time. On the other hand, each year he tends to appear as a secondary author on more papers than the year before. This is likely due to the growing number of his former students in research universities, as well as to academic networking.

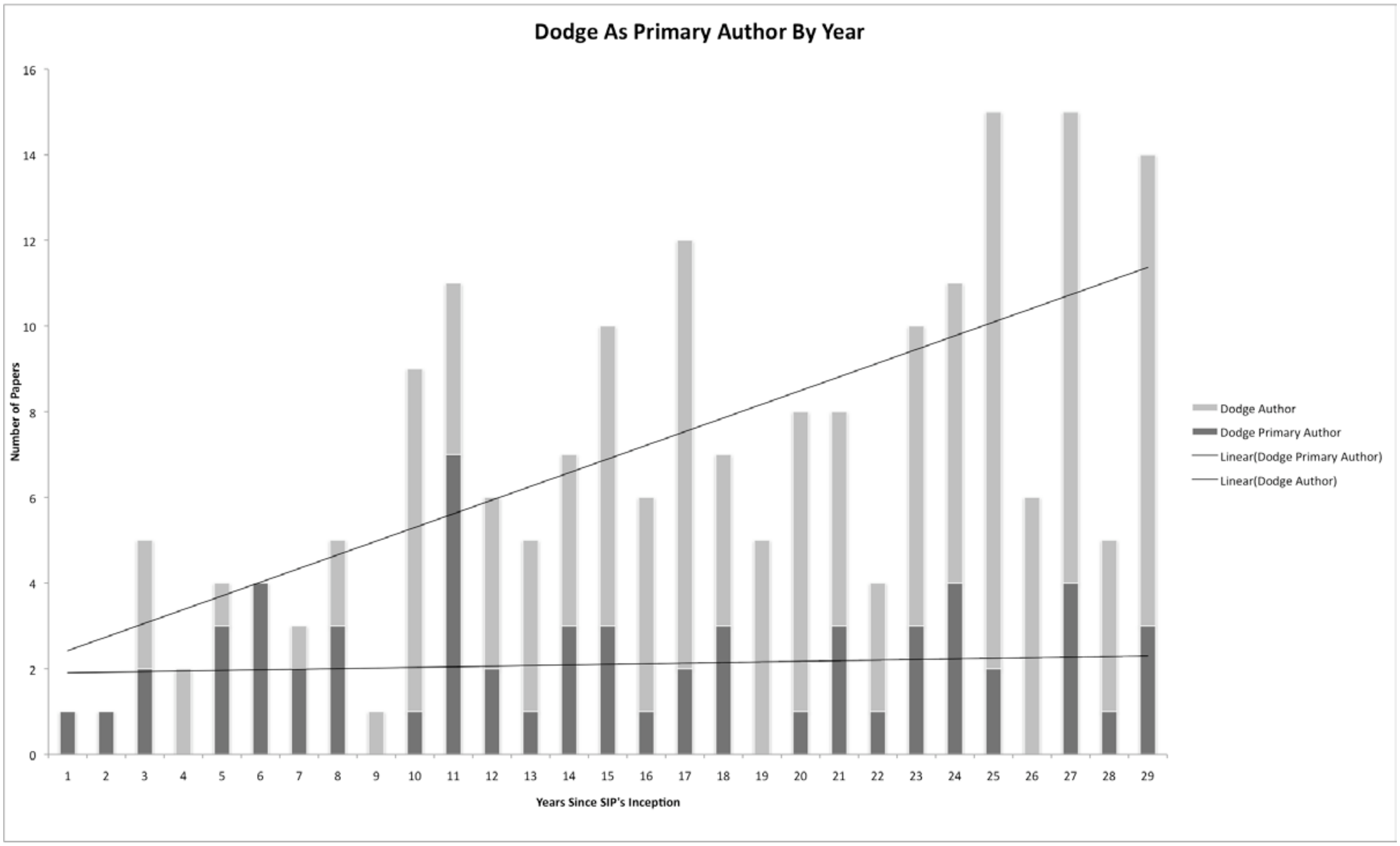

Figure 12. Dodge's authorship, using information from Dodge's CV, as of 2009.

Third, I would like to look at SIP research more broadly, in terms of the history of citations. Figure 13 demonstrates the growing number of papers that cite or discuss SIP theory with each year. For this data, I used Google Scholar. I searched for the complete phrase "social information processing," and used search filters to calculate the number of papers published in each year between 1991 and 2009 that use that phrase in their citations. An initial search of "social information processing" brought 13,800 results. In order to address the possibility that other similarly termed theories could be confounding this result, I added, "Dodge" to the search. This generated over 5,200 papers. Adding "Huesmann" returned about 1,000. For comparison, adding "Chomsky" generated 143 papers and adding "Quine" generated 78 papers. In the case of adding "Jurafsky," a Stanford Professor who studies information theory and computational linguistics, Google Scholar generated only two papers. Granted that this does not eliminate the possibility of confounds, it does lend support to the supposition that this figure reflects the growth of the SIP research tradition and not a something else sharing a similar name.

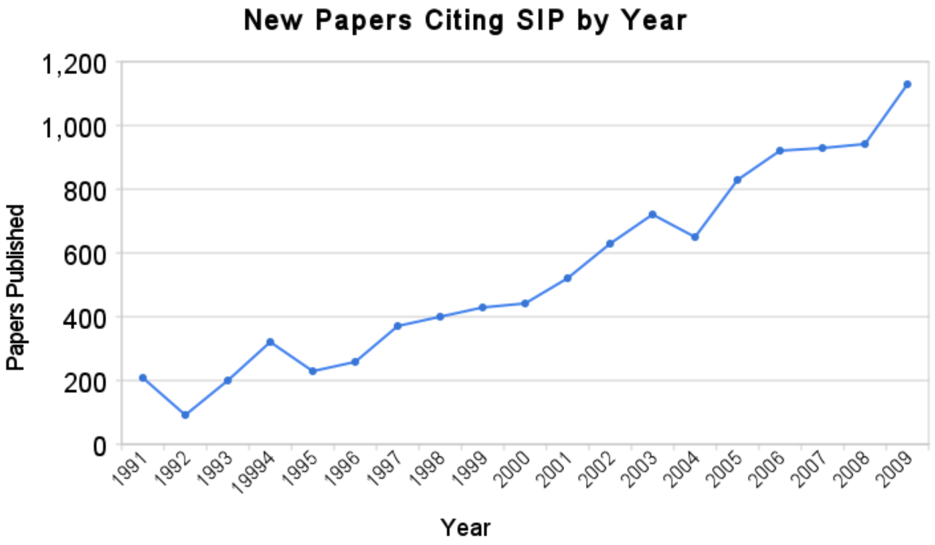

Figure 13. Citations of "information processing theory" on Google Scholar, as of early 2010.

Figure 13 illustrates the accelerating growth of SIP research, and corresponds with the accelerating growth seen in Figure 12. Not only does Figure 13 illustrate accelerating growth, but also illustrates that the rate of acceleration increases at about 2000. This can be difficult to see visually, so I divided the data at the year 2000, which is conveniently midway between 1991 and 2009. I confirmed the change using a Manual Chow Test.[79] The slope of the line of best fit between

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[79] The Chow Test is a statistical method used to indicate whether the coefficients in two linear regressions of different data sets are equal.

1991 and 2000 was 34.12 degrees, compared to 67.67 degrees between 2000 and 2009. The Chow Test gave a P value of .007229, which indicates that the odds are 99.28% that this result is not due to chance.

This increase in the rate of citation can indicate a number of things. Once possibility is that it reflects the time period in which the first generation of Dodge's students became established researchers and founded their own labs. Another possibility is that the research community simply hit a threshold in the number of authors involved in SIP research that lead to a boom in mutual citations. Still another possibility is that external factors, such as the popularity of ADHD diagnoses, lead researchers to cite this work. Many of the 13,800 citations link directly to Dodge's work, with his 1994 collaboration with Crick alone receiving nearly 2,000 citations. Perhaps another prominent author discussed SIP theory (and perhaps the 1994 model in particular) in 2000, and this brought it to the attention of more researchers. It is also possible that this change was part of a larger trend in the culture of citation, perhaps linked to the growing role of the Internet in research activities. These questions would likely be fruitful topics for future inquiries.

The change around 2000 also correlates roughly with the growing empiricism in the content of SIP theory. It is unclear which direction the causal arrow points to. It is possible that as SIP became more empirical, more researchers found it worthy of discussion in their own work. On the other hand, perhaps the changing theoretical character of SIP research stems from the influence of new researchers becoming involved.

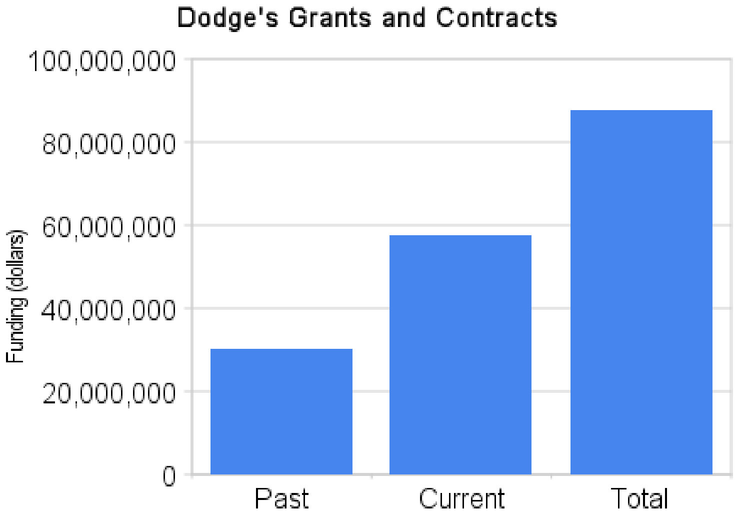

Figure 14. Content taken from Dodge's CV.

Another domain where quantitative methods might help is in research funding. Fortunately, for my purposes, Dodge includes a list of his grants and contracts in his curriculum vitae. Based on these figures, his past grants and contracts amount to $30,199,900. His current funding amounts to $57,753,299, which makes a total of $87,953,199. See figure 14 for a graphic display of these figures. This funding is truly exceptional for a psychology researcher, although perhaps it is not surprising considering the success of Dodge's research program. Much of the funding comes form multi million dollar grants from the National Institute for Mental Health, including one for $18,000,000 to study the prevention of behavioral problems among adolescents. Figure 15 displays the major sources of Dodge's funding.

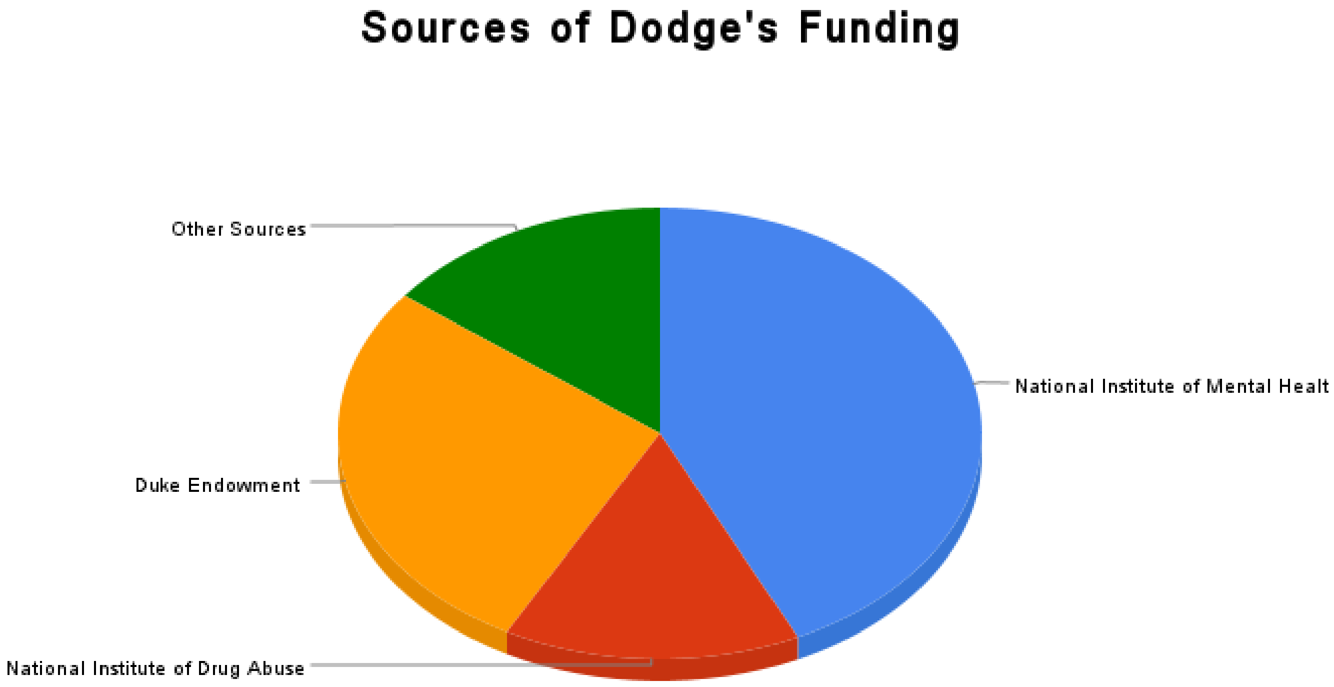

Figure 15. Content taken from Dodge's CV.

Because most researchers in the field to not advertise their funding so openly, I turned to Research Crossroads to obtain funding information about other aggression researchers.[80] According to Research Crossroads, Dodge has received $39,096,687, which only includes funding through grants—this appears to be consistent with his curriculum vitae. Figure 16 compares his grants to that of several other prominent researchers in related areas of study. These researchers include:

- Gregory Pettit of Auburn University. Research includes social information processing and aggression.
- Klaus Miczek of Tufts. Research includes the neurobiology of aggression.
- George Barlow of UC Berkeley. Research includes aggression in animals.
- Neal Simon of Leigh University. Research includes the neurochemistry of aggression.
- Bruce Svare of SUNY at Albany. Research includes hormonal and psychobiological determinants of aggression.
- Peter Giancola of University of Kentucky. Research concerns alcohol and aggression.
- Josephine Johns of University of North Carolina Chapel Hill. Research includes drug use and aggression.
- Richard Melloni of Northeastern University. Research includes the neurobiology of aggression.
- Richard Wrangham of Harvard University. Research includes aggression in primates.
- Rowell Huesmann of University of Michigan. Research includes social information processing and aggression.
- Michele Ward of University of Southern California. Research includes prenatal smoking and aggression in twins.
- Allan Meltzer of Carnegie Mellon. Research includes public policy and economics.
- Peter Diamond of Massachusetts Institute of Technology. Research includes psychology and public policy

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[80] Research Crossroads is an organization that assembles grant data using the Freedom of Information Act. Their sources of information include the National Institute of Mental Health, the National Science Foundation, Department of Defense Congressionally Directed Funds, and Grants.gov. Research Crossroads provides a searchable database of grants, as well as their recipients. Using this tool, it is possible to see where researchers in almost any field obtain their funding, although this does not include private contracts.

- Howard Kaplan of Texas A&M. Research includes the sociology of aggression.
- Dolf Zillman of Indiana University. Research includes aggression and education.
- Carroll Izard of the University of Delaware. Research includes emotion processing and aggression.
- Jerald Herting of Stanford University. Research includes education and high-risk children.

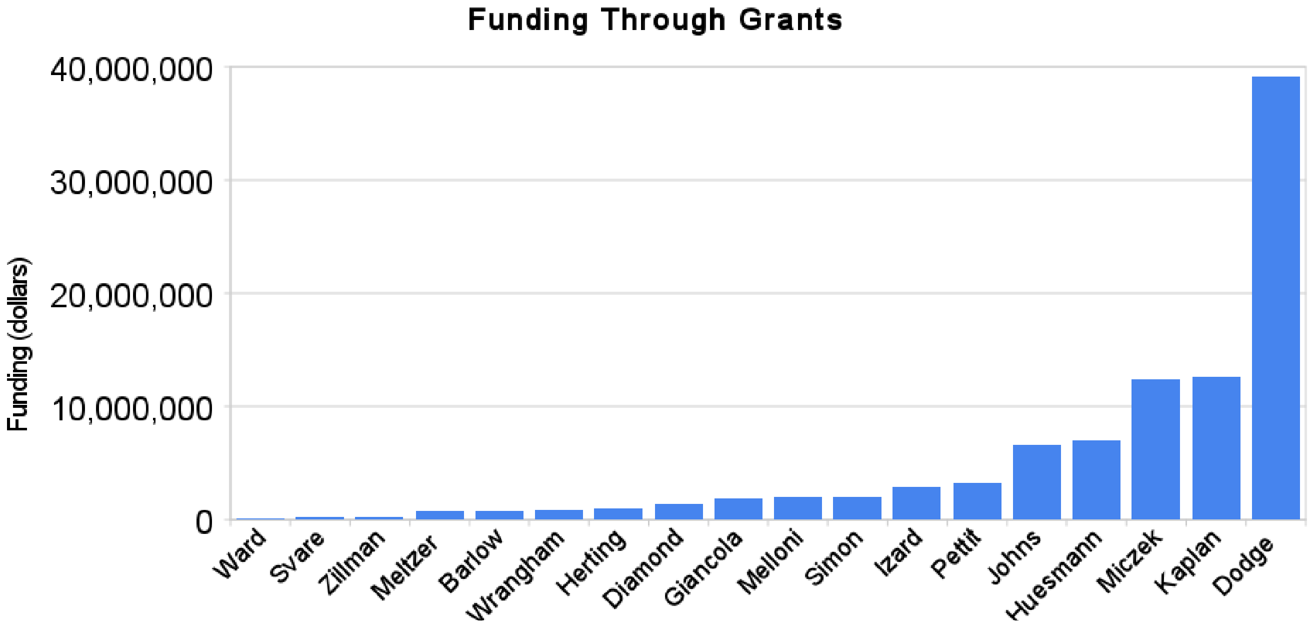

Figure 16. Grant data from Research Crossroads.

Not only was Dodge's work received well by the academic community, but it was also nurtured with absolutely exceptional financial support, which may have encouraged increasing numbers of researchers to join his research program over time. This remarkable funding is due in part, no doubt, to his involvement in public policy applications of his research, which relatively few other researchers engaged with. Through SIP theory, Kenneth Dodge became a veritable titan among aggression researchers, and his empire continues to grow to this day.

3. Modeling Scientific Development

Philosophers often conceive of science as an essentially epistemic endeavor: the application of rigorous rules of inference to a set of observations, with the goal of creating positive knowledge. The philosophy of science is itself typically approached as a special type of epistemology, concerning the structure of positive scientific knowledge. Upon adopting such an approach, one immediately confronts the questions of how and why, unlike other forms of knowledge, the lessons of science change and presumably, improve over time. Much ink has been spilled over these questions, and numerous philosophers of science have proposed models to account for and evaluate scientific development. In this chapter, I will appraise the models of development espoused by Kuhn, Lakatos, Laudan and Hacking with reference to the history of SIP theory.

SIP theory makes a valuable case study in scientific development for a few reasons. One is that SIP theory changes significantly over time, and in some ways, seems to improve. Another is that SIP theory is not prototypically scientific, but nevertheless fits within the stated domains of application attributed to many models of development. A third is that unlike many popular historical examples, SIP theory is both well documented and accessible, which makes a thorough discussion possible. This chapter focuses on four central topics: 1) units of analysis, 2) continuity, 3) maturity, and 4) progress. Each of these topics plays a crucial role in models of scientific development, and each can be brought to light by the history of SIP theory. I will primarily relate these topics to the models of scientific development proposed by Kuhn, Lakatos and Laudan, but I will also address the views of Hacking, Foucault, and Metaxopoulos on specific points.

My primary criterion of evaluation will be descriptive adequacy, and I begin with the assumption that a purely descriptive model of scientific development should be accurate in describing the relevant aspects of SIP theory. In places, I rely on the more contentious assumption that a successful normative model of scientific progress should describe the progressive aspects of SIP theory. I will elaborate these assumptions and address objections in the next chapter.

3.1 Units of Analysis

Different models of scientific development generally concern different units of analysis, and it would be a mistake to assume that these units were directly commensurable. In this section, I will outline four widely influential units of analysis that appear in the relevant literature and discuss their applicability to the history of SIP research. My discussion will include Kuhn's paradigm, Lakatos's research programme, Laudan's research tradition and Hacking's "elements." Finally, I will address my own use of the term "research program," which appears throughout Chapter 2. By no means does this constitute an exclusive set of possible units of analysis, or even a comprehensive list of those that have appeared in philosophical writing about scientific development. In particular, I will not discuss pre-Kuhnian views on the proper units of analysis, which I do not believe are relevant to more contemporary discussions in the philosophy of science.

3.1.1. Kuhnian Paradigms, Disciplinary Matrices, and Exemplars

In the initial characterization of "paradigms" in *The Structure of Scientific Revolutions*, Kuhn describes them as achievements that were "sufficiently unprecedented to attract an enduring group of adherents away from competing models of scientific activity… [and] sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve." As examples, he cites Aristotle's *Physics*, Ptolemy's *Almagest*, Newton's *Principia* and *Opticks*, Franklin's *Electricity*, Lavoisier's *Chemistry*, and Lyell's *Geology*. According his analysis, each of these paradigms provided a conceptual and methodological framework for subsequent scientific practice, which included "law, theory, application, and instrumentation."[81]

It is clear that what Kuhn has in mind is a body of ideas that determines the norms of subsequent scientific practice and content, but it is not always clear how he means to apply this concept. It is never made entirely clear whether the notion contains a strong normative component, or exactly which fields of study he hopes to characterize with it, since most of his examples come from early physics and chemistry, and rarely (or never) from the biological sciences or social sciences. Moreover, throughout SSR, Kuhn uses the word to signify a variety of different notions—Masterman (1970) counted 21 distinct senses of the word “paradigm” within SSR. For these reasons, I will turn to the secondary literature to characterize the notion of the paradigm more precisely.

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[81] SSR. Pg. 10.

Godfrey-Smith distinguishes between two primary senses of the word “paradigm” that pertain to Kuhn’s model of scientific change. The first is the *broad* sense of “paradigm,” which refers to a package of ideas and methods, which combine to compose a view of the world and a way of conducting science. This type of paradigm is characterized by the universal consensus among researches regarding the proper norms surrounding science. Alternatively, Kuhn also uses the *narrow* sense of “paradigm,” which refers to a specific element or exemplar that serves as a model for subsequent scientific activity. In many cases, a paradigm in the narrow sense will be a part of a paradigm in the broad sense.[82]

Kuhn himself was acutely aware of the ambiguities in SSR, and made numerous attempts to clarify his ideas in later writings. Ultimately, the terms “disciplinary matrix” and “exemplar” came to replace the broad and narrow senses of “paradigm,” respectively.[83] The new terms are used somewhat differently than their predecessors,[84] but these differences are not pertinent to my discussion of SIP. Hoyningen-Huene (1993) describes four primary components to disciplinary matrices in Kuhn’s later work: 1) symbolic generalizations, 2) models, 3) values and 4) exemplary problem solutions. Symbolic generalizations include purely mathematical reasoning, and models include both heuristic (analogical) models as well as those that express a deeper metaphysical conviction about a subject. “Values” refers only scientific values pertinent towards epistemic standards,[85] and not values more broadly. In subsequent sections of this chapter, I will frame Kuhn’s model of scientific change in terms of disciplinary matrices and exemplars as a matter of clarity. Hoyningen-Heune also states that the scientific community engaged in a disciplinary matrix must have similar educations, similar professional initiations, be absorbed in the same technical literature, and draw many of the same lessons from it. [86]

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[82] Godfrey-Smith pg 77.

[83] Kuhn, T. 1974, “Second Thoughts on Paradigms”, in *The Structure of Scientific Theories* edited by F. Suppe, Urbana: University of Illinois Press: 459-82.

[84] E.g. the requirement of universal consensus in paradigms was dropped in Kuhn’s later discussion of disciplinary matrices.

[85] The set of general scientific values is described as including: accuracy, consistency, scope, simplicity and fruitfulness. Other possible values include naturalness, plausibility, explanatory power, the unity of science, and so forth.

Now, I would like to discuss the applicability of these concepts to SIP theory. The first question is whether Kuhn's concepts, which were clearly designed to characterize the hard sciences, should be applied to describe SIP theory at all. While Kuhn is not explicit about the limits of his analysis, Hoyningen-Heune[87] infers from Kuhn's arguments that the notion of paradigms extends to include the systematic social sciences, which would undoubtedly include SIP theory. On a disciplinary basis alone, I see no reason for precluding SIP from Kuhnian analysis.

Does SIP theory have an exemplar? One clue comes from academic citations—nearly every paper in the history of SIP theory cites Dodge 1980. This modest academic publication was hardly on par with Aristotle's *Physics* or Newton's *Principia* in terms of either its insights or depth, but it did share a relevant set of features. That is, it certainly served as a methodological basis for most of the subsequent work regarding SIP, and it established an operational definition of aggression that was not only novel at the time, but remains influential today. Other papers from the early 1980s, and perhaps leading up to the widely popular 1986 model, further clarified the concepts and methodologies that would later become foundational. I believe that Dodge 1980 and the body of papers published over the next few years provide as good an example of a Kuhnian exemplar as one is likely to find in modern science.

Is SIP theory a disciplinary matrix? Once again, I would argue that the answer is yes. There is hardly any attempt at symbolic generalization in SIP theory until about 2008, but the other components of disciplinary matrices can all be seen at work in SIP research. SIP has used models both as analogical heuristics and as ontological representations, and in both cases, there appears to have been agreement within the community of researchers. The scientific values of SIP theorists also appear to have been the subject of general agreement. This is especially noteworthy, since the validity of some of the experimental methods is questionable (particularly in the very early studies), and there appears to have been no substantial criticism brought against them from within the community. Moreover, the researchers tend to demonstrate a common interest in refining the statistical reliability of their operational definitions rather than in the many alternative routes their research could have taken. Similarly, since about 2000, the research community as a whole seems to have adjusted towards a somewhat more empirical and causally concerned attitude. There is also little reason to suspect that the researchers would be operating with different notions of exemplary problems solutions.

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[86] pg 9

[87] pg 4

Regarding the research community itself, Kuhn's analysis appears applicable once more. The researchers shared similar educations, and many of them studied under the tutelage of Dodge himself. The researchers appear to have been responding to the same scientific literature, given their citations, and they appear to have taken the same lessons from them, given the lack of debate or dissent within the SIP research community.

3.1.2. Lakatosian Research Programmes

In *the Methodology of Scientific Research Programmes*, Lakatos proposes the "research programme" as the basic unit of appraisal and analysis in science. A research programme consists of a series of related theories in temporal sequence. It contains a "hard core" of assumptions, which are invariant over time and immune to modification. It also contains a "protective belt" of auxiliary hypotheses, which can be modified in the face of recalcitrant observations. Finally, the research programme contains a "heuristic," which is an internal set of problem-solving techniques (often involving mathematical techniques) that turns anomalies into positive evidence.[88]

Lakatos saw individual theories as essentially static aggregates of knowledge, and envisioned a research programme as a dynamic historical entity that exists as a series of stages, such that only the core of their theoretical body would remain stable.[89] Laudan, characterized research programmes as "super-theories."[90] In addition to its descriptive dimension, the research programme was also understood to be the primary unit of appraisal in evaluating the rationality of theory choice within the scientific community.[91] Research programmes are thought to exist contemporaneously with rival traditions, and can only be evaluated relative to their competitors. I will address the issue of evaluation later in this chapter.

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[88] Pg. 4.

[89] Larvor, 51.

[90] Laudan pg 76.

Like Kuhn, Lakatos never makes it entirely clear how the notion of research programmes applies to historical traditions. While Lakatos draws from a handful of classic examples in the history of physics (e.g. Newtonian mechanics), his work decidedly focuses on the logic of justification as opposed to describing actual scientific practice.

Despite the ambiguity noted above, it seems reasonable to assume that the term "research programme" picks out entities roughly analogous to those picked out by Kuhnian disciplinary matrices.[92] Nevertheless, a few important features separate research programmes from disciplinary matrices:

- At any given time, multiple research programmes compete within a field.
- Research programmes do not necessarily constitute "world views," as disciplinary matrices are thought to do.
- Similarly, research programmes are not stable epistemic methodological perspectives. While their core assumptions remain fixed over time, the collection of auxiliary hypotheses can shift to accommodate new discoveries and anomalies.
- Research programmes are a narrower concept in the sense that they include only the hypotheses, methodologies and content of a theory, but seem to exclude scientific instruments and other physical artifacts that relate to practice.

It is unclear how well the notion of a "research programme" applies to SIP. While SIP is often presented as a "theory" in the psychological literature, it is not obvious that it qualifies as an "individual" theory in Lakatos' sense. In particular, SIP "theory" changes substantially over time, and can be broken into a series of more or less stable phases. To this extent, SIP theory is really a series of theories, and it does appear to correspond to the notion of a "research programme."

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[91] According to Godfrey-Smith, pg. 103, this was a reaction to the perceived epistemic chaos in science implied by Kuhn's writing.

[92] Godfrey-Smith makes this claim pg. 104, and describes Lakatos' analysis as applicable to all of science, from "physics to the social sciences."

The applicability of the term "research programme" to SIP theory is complicated by the fact that Lakatos does not specify what types of competition must exist between research programmes. It seems reasonable to assume that Lakatos is more interested in describing competition for correctness, as opposed to competition for funding.[93] It less clear however, how Lakatos would approach competition for "active" acceptance among researchers.[94] In the case of SIP theory, competition for "correctness" is minimal—with the exception of a few early papers reacting to prior wok in Drive theory, SIP theory is at least potentially consistent with most research in psychology, genetics and neurobiology. Even its outstanding rivals (e.g. social learning theory, or perhaps strands of ADHD research) differ more in focus than in content. We can see this in the fact that no systematic criticisms have been leveled against SIP from other programmes, as one sees in typical examples of competition between the content of multiple theoretical approaches.[95]

On the other hand, SIP theory certainly does compete with many rivals in psychology, genetics and neurobiology in terms of funding and active acceptance. The graphs in chapter 2 testify to its success in both domains. It seems that competition does in fact play an important role in the development of SIP theory, but it is not clear that the competition that actually occurs is what Lakatos had in mind. The fact that SIP theory does not generally compete for correctness[96] hardly makes it unique among other sciences, and it seems that in this respect, Lakatos' view of competition may be too narrow to be descriptively useful.

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[93] This certainly seems to be true with reference to classic examples from the history of science, such as the competition between geocentrism and heliocentrism.

[94] By active acceptance, I mean the theory's success in attracting researchers, as opposed to the number of researchers who might consider it true.

[95] For instance, the criticisms and debates surrounding the Copernican revolution, the fall of behaviorism to cognitivism, or even in the rise of connectionism in the 1980s.

[96] I suspect that most researchers in relevant fields would agree that the content of SIP theory is essentially correct, although they may well disagree that it the best way to study aggression relative to their own areas of focus.

Additionally, Lakatos posits a stable core of assumptions in every research program, such as Newton's universal laws of motion, or heliocentrism in Copernican cosmology. If we approach SIP with these analogies in mind, it seems like the set of models of information processing would be a likely candidate for the "core," since they appear to be immune to empirical findings. This is problematic, however, because the basic model at the heart of SIP is not stable at all, and shifts over time as the interests and priorities of the researchers change. Perhaps the "core" is the very assumption that deviant behavior is caused by deviant cognition, but this assumption is not specific to SIP at all. My intuition is that SIP theory does have a core at any given time, but that this core is not necessarily stable over the long term, as Lakatos' model would require.

While the research programme does not apply to SIP particularly well, I will proceed to analyze other aspects of Lakatos' models of scientific change in connection to SIP in later portions of this chapter.

3.1.3 Laudan's Research Traditions

In *Progress and its Problems*, Laudan builds from Lakatos' notion of research programmes to develop the concept of the "research tradition." Laudan remarks that every intellectual discipline, scientific or not, has its own traditions. These traditions generally share common traits:

- Each research tradition contains multiple specific theories that may be contemporaneous or sequential
- Each research tradition makes metaphysical and methodological commitments that differentiate it from other tratidions.
- Each research tradition goes through multiple formulations over long periods of time.[97]

Examples of research traditions vary in scope, from nominalism in philosophy to behaviorism in psychology, and from capitalism to Cartesian physics. Research traditions provide guidelines for the development of specific theories and hypotheses. They outline the types of theoretical entities that can be discussed, as well as the general modes by which they may interact. They also specify legitimate modes of inquiry. Research traditions as a whole are neither explanatory or predictive, or even testable. As a working definition, Laudan distills the notion of the research tradition as "a set of general assumptions about the entities and processes in a domain of study, and about the appropriate methods to be used for investigating the problems and constructing the theories in that domain."[98]

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[97] Laudan (1978) pg 79

Laudan's research tradition differs from Lakatos' units of analysis in substantial ways. As Godfrey-Smith [99] observes, Laudan abandons Lakatos' emphasis on logic, as well as the notion that each new theory in a sequence should encompass a broader domain of application. More importantly, Laudan posits that research traditions have "cores," but that they are not necessarily stable over time. This topic will receive more attention in subsequent sections of this chapter. Laudan's model is firmly rooted in historical cases, and in addition to a normative component relevant to the rationality of theory-choice, his notion of the research tradition is meant to be descriptive of most scientific practice as it actually occurs.

Unlike the research programme, the research tradition is readily applicable to SIP. This is because Laudan does not emphasize the stability of a tradition's core, which is one of the issues that proved problematic for Lakatos' programmes. Laudan also drops competition as a necessary condition for traditions, which spares him from the ambiguities attached to Lakatos' account. Moreover, Laudan presumably does not privilege competition for "correctness" over other types of competition.[100] For these reasons, Laudan appears to be successful in avoiding the problems that prevented the notion of research programmes from applying directly to SIP research.

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[98] ibid pg 81

[99] Godfrey-Smith (2003) pg 108

[100] It seems that two theories with logically inconsistent claims might exist within the same research tradition, which suggests that if two traditions compete, it would not be for the sake of consistency at the content level.

3.1.4 Hacking's Elements

Hacking (1992) offers an account of the development of the so-called laboratory sciences. He limits his analysis to traditions that study energy and matter in controllable and isolatable conditions,[101] but much of his analysis is readily applicable to experimental psychology. Hacking rejects the idea of classifying units of science in a strict taxonomy. Rather, he uses the three coarse categories of "ideas," "things" and "marks." Ideas include: 1) questions, 2) background knowledge, 3) systematic theory, 4) topical hypotheses, 5) modeling of the apparatus. Things include: 6) the target, 7) the source of modification, 8) detectors, 9) tools, 10) data generators. Marks include: 11) data, 12) data assessment, 13) data reduction, 14) data analysis, 15) interpretation.

Hacking views scientific activity as the interplay of many traditions,[102] comparing it to a rope with many strands.[103] Even if a break occurs in one tradition, the relative stability of the others can create the impression of continuity in that field of study.

It seems as if Hacking's units are well suited to describe SIP research. All fifteen elements of laboratory science may be found in SIP research, and it seems that the psychology laboratory is, if not particularly well controlled or isolated, analogous in structure to the physics or chemistry laboratory. Moreover, SIP can easily be approached as a set of interwoven but distinct traditions. For instance, SIP researchers inherited theoretical connections between frustration and aggression from Drive Theory, research goals from social workers, formal models from mental chronometry, and so forth.

3.1.5: My use of the term "Research Program"

Throughout Chapter 2, I use the term "research program" to casually designate the set of researchers, ideas, methods, instruments, and so forth that compose the internal history of SIP. That is, it is intended purely as a shorthand expression for "what goes into *this* history." I do not intend to generalize this unit of analysis to other cases, or to seek justification from prior philosophical principles. The present section is included to eliminate possible confusion with Lakatos' "research programmes," for which I will always use British spelling.

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[101] Hacking (1992) pg 36

[102] E.g. theoretical, experimental, instrumental traditions.

[103] Ibid pg 41

3.2 Continuity

Most models of scientific change rely on the notion of discontinuity in separating temporally ordered traditions from one another, and similarly entail a notion of continuity that holds individual traditions together as legitimate units of analysis. In this section, I will discuss different senses of the term "continuity" as they relate to SIP. This includes notions of continuity and discontinuity between disciplinary matrixes, programmes, and traditions and their predecessors, as well as continuity between the individual theories that compose them.

3.2.1 Kuhn: Continuity as Commensurability

In the *Structure of Scientific Revolutions*, Kuhn distinguishes between normal science and revolutionary science. To put it roughly, normal science occurs within a disciplinary matrix, and works within the framework provided by an exemplar. Most scientific practice occurs as normal science. Revolutionary science occurs during periods in which one disciplinary matrix replaces another, and foundational questions are re-evaluated by the scientific community.

3.2.1.1 Revolutionary Science

In the Kuhnian model of scientific change, science is a non-cumulative, discontinuous process. The reason for this is incommensurability—literally, that no disciplinary matrix is comparable to its predecessors in terms of accuracy, truthfulness, or any other standard measure. That is, a new disciplinary matrix is no more comparable to its predecessor than to one from an unrelated field. Godfrey-Smith articulates two reasons for incommensurability that pertain to my analysis: 1) communication is problematic because researchers in different matrices effectively speak slightly different languages and 2) different matrices use different standards of evidence and argument.[104] The standards of evidence at play in SIP are not unusual relative to contemporary psychological sciences, so my discussion will focus on the former reason. In this section, I will not discuss the tenability of incommensurability as a general principle or as it pertains to theories of meaning, but only as it applies to the history of SIP theory.

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[104] Godfrey-Smith (2003) pg 91.

I will look at two ways of thinking of incommensurability. The first is what I will call "strong" incommensurability, which Kuhn seems to argue for in the controversial tenth chapter of *The Structure of Scientific Revolutions*—the idea that different disciplinary matrixes entail wholly distinct "worlds" of scientific discourse. Not only do scientific revolutions create new sets of problems to be solved, but also fundamentally alter the scientific community's perception of the world. The creation of a new disciplinary matrix creates a systematic gestalt switch, analogous to an ambiguous image that may appear to be a duck to some viewers and a rabbit to others.[105] This means that when Lavoisier talks of "oxygen," he refers to something that his predecessor Priestly is incapable of seeing, for it has no corresponding concept in the prior disciplinary matrix. Because of this incongruity, reference does not carry over across revolutions, and every disciplinary matrix starts anew. Strong incommensurability also demands that scientific traditions be approached holistically, such that the alteration of a certain subset of elements in a disciplinary matrix results in the creation of a new one altogether.[106]

Most of the examples that Kuhn uses to argue for incommensurability involve one disciplinary matrix replacing the other to become dominant within a field (e.g. Ptolemy and Copernicus, Aristotle and Newton, Priestly and Lavoisier). Unlike these cases, this does not occur with SIP theory. It is difficult to say that SIP is incommensurable with drive theory or social learning theory—these fields used vastly different heuristic models, but share many assumptions and methodologies as well. The fact that one attributes aggression to a processing deficit and the other attributes it to mimicry hardly seems like grounds the claim that they entail different and incomparable "worlds."

Consider the question of SIP theory's commensurability to a contemporary genetic account of aggression. While it seems fair to say that research in the two fields cannot be compared directly, it is also easy to imagine the two disciplinary matrices using the same heuristic models, or even merging. For instance, it would not be surprising to see the use of genetics to explain the information processing deficits portrayed by SIP theory. Assuming that strong commensurability holds, a merger of the two fields would entail a new "world," or perhaps one world simply being subsumed into another. Perhaps this is true in terms of the semantic web entailed by the new body of theory, but it seems like an implausible account of the actual psychology of the researchers—there is no reason to suspect that a gestalt switch would mediate the change in theory.

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[105] SSR pg 111.

[106] That is why, when Copernicus moved the center of the cosmos from the earth to the sun, he created a new disciplinary matrix, and a "world" altogether different from the one inhabited by adherents of the Ptolemaic model. Presumably only "fundamental" changes in the matrix should result in such a shift, because without some criterion for significance, every new fact or solution to a puzzle would entail a new worldview.

If strong incommensurability were applicable to the cross-matrix context of SIP theory, one would expect to see the signs of miscommunication and confusion in the interactions between researchers from different fields. Simply put, this confusion does not appear to occur frequently. In fact, SIP theory grows out of the intersection of developmental psychology, social psychology and cognitive science—a feat that one might think impossible without some degree of effective communication across matrices. Not only that, but SIP continues to drift between these domains throughout its history, as with its 1994 movement towards connectionist modeling and the studies in the 2000s concerning prenatal alcohol exposure. Perhaps SIP researchers somehow talked past their colleagues in subtle ways that do not appear in my coarse historical analysis, but this would require that we introduce a different notion of incommensurability.

The second approach that I would like to discuss in this section is "weak" incommensurability, which allows for matters of degree on a case-by-case basis. This position makes a commitment to holism, but not to the existence of volatile worldviews. What makes one disciplinary matrix more or less commensurable to another is the particular combination of changed elements in the matrix and their particular connections to other elements. In this view, there can be terms that have only "slight" differences in use across disciplinary matrices, which allows for partial communication, but there may also be situations where the notion of changing worldviews would be accurate. That is, a holistic comparison between matrices may be difficult, but is not impossible. Hoyningen-Heune puts this in terms of the possibility of incommensurable theories having "roughly the same object domains."[107]

In terms of SIP, the weaker sense of incommensurability is relatively tenable. Simply put, it offers potential solutions for the problems found in the application of strong incommensurability—that is, the fact that SIP seems to be partially, though not entirely comparable to many contemporary and earlier disciplinary matrices. While there is no simple way to compare SIP to a genetic or neurological disciplinary matrix, there may nevertheless exist ways of analyzing the particular strengths and weaknesses of the different approaches and gaining some evaluative power. However, the actual performance of such an analysis would be no simple task, and it is unclear how one would actually go about it. Moreover, philosophers such as Fodor and LePore (1992) have criticized efforts to develop moderate forms of holism that do not commit to the "strong" view. These debates lie outside of my current scope, but it should be noted that the tenability of weak incommensurably depends on how one understands the controversial topic of semantic holism.

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[107] Hoyningern-Heune (1993) Pg. 219

3.2.1.2 Normal Science

By definition, normal science requires a disciplinary matrix to set the norms of scientific practice. Kuhn never discusses the role of commensurability within normal science, and he likely assumed that any particular disciplinary matrix is commensurable with itself at times T1 and T2. In the abstract, this assumption has an intuitive appeal, but it is not obvious in the case of SIP.

As a singular disciplinary matrix, one would expect that SIP theory is fully commensurable with itself over time. Yet, many changes occur within its history, some of which are foundational—for instance, the function of the core models shifts considerably from 1984 to the present, priorities in problem solving change,[108] and even the relevant class of theoretical entities seems to shift.[109] Given these changes, one might expect that SIP theory at 1980 would be incommensurable with SIP theory at 2010. While there is some sense in which the worldviews of Dodge, Crick and Fontaine may have shifted, no systematic gestalt switches appear to have occurred within their careers, and nothing indicates that rational discourse between Dodge at 1980 and Dodge at 2010 would be impeded by the use of different "languages."[110] Moreover, there is an evident continuity within the research tradition that includes not only the theory itself, but also the instruments of experimentation, the research institutions, and the personalities involved.

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[108] One instance of this is the increased attention to causal questions during the 2000s, which had been largely ignored before this time.

[109] This is evident in the shift revealed in the change in focus from the "aggressive boy" to "aggressive behavior and at risk children." The change in language corresponds to changes in experimental design, sampling and even theorizing. Going well beyond the explanadum, this change included a shift away from ontological commitments to the existence of particular cognitive processes.

A weaker form of incommensurability might allow one to say that SIP at 1980 differs from SIP at 2010 without entailing a wholly separate worldview;[111] this approach has obvious advantages in accounting for the apparent lack of confusion and miscommunication among SIP researchers. Even if one holds the weaker notion of incommensurability, however, this would entail that SIP has a "slightly" different disciplinary matrix at different points in its internal history—essentially, that something like a small-scale paradigm shift occurs in the absence of a scientific revolution. This would challenge the very notion of static normal science as Kuhn presents it.

Kuhn describes scientific revolutions through an analogy to political revolutions—brief periods of upheaval and fundamental change. Perhaps revolutions happen during normal science as well, but occur quietly, best characterized with an analogy to democratic elections. In the normal science observed in SIP, fundamental changes occur gradually, without violent factions, and sometimes implicitly. Though it does change over time, we may nevertheless think of SIP as belonging to the same disciplinary matrix throughout it its history, much as we are inclined to think of the United States of the late eighteenth century as being the same country as today. This view of change bears some resemblance to the notion of the refinement of a disciplinary matrix, but differs in that it involves a problemshift—that is, when fundamental assumptions shift, researchers at time T2 are no longer solving the same types of puzzles as at time T1.

3.2.2 Lakatos and Metaxopoulos: Continuity as Similarity

While Lakatos does not address it directly, an interesting conception of continuity emerges in *The Methodologies of Scientific Research Programs*. To begin with, Lakatos characterizes the development of a research programme as the accumulation of new facts and empirical growth,[112] which suggests a notion of internal coherency and consistency, not unlike what Kuhn assumes of disciplinary matrices. My focus in this section, however, will be the notion of continuity over long periods of time, and potentially across research programmes.

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[110] That is, no more so than any other person at 1980 and 2010.

[111] In this case, it is plausible that Dodge might reflect on his career and say, "I suppose we saw the world a little bit differently then." This type of change is presumably not what Kuhn has in mind.

Unlike Kuhnian disciplinary matrices, it is not always appropriate to compare one of Lakatos' research programmes to its immediate predecessor. The reason for this is that research programmes do not replace each other sequentially, but rather coexist and compete, potentially over long periods of time. What may appear to be a "new" programme may simply be the revival of an older one following a period of relative unpopularity. This conception of research programmes bears a striking resemblance to the Hegelian dialectic model of history, which likely influenced Lakatos' intellectual development.[113] Using the Lakatosian model, one might expect to find roughly commensurable historical antecedents for many popular contemporary theories (or sequences of theories) as a simple matter of the true longevity of research programmes.

Detailed examples occur infrequently in Lakatos' writing, but Metaxopoulos (1989) describes what I take to be a best-case scenario for the applicability of the Lakatosian sense of continuity at a large timescale: scholasticism and science in the early 17th century.[114] Metaxopoulos argues that late medieval epistemic and "problem-posing" traditions anticipate the methodological characteristics of the 17th century scientific revolution, and that this continuity was broken by the humanism and occultism of the Renaissance. In this case, what appears to be a series of three sequential traditions is actually the interplay of two larger research programmes over the course of about two centuries. In this case, continuity is not a matter of strict commensurability, but seems to be much closer to the degree of similarity between two theories separated by time. I do not mean to commit to the historical validity of this example, but it is worth asking—does SIP fit into such a pattern with reference to a broader historical scale?

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[112] Lakatos (1980), pg. 112.

[113] Larvor's biography of Lakatos discusses the role of Marxism in his life, including his time spent working under the Hegelian-Marxist philosopher Gyorgy Lukacs. See Larvor pg 4. Hacking (1979) emphasizes the Hegelian influence on Lakatos' implicit theory of representation.

[114] Metaxopoulos (1989) Pg. 211. Metaxopoulos' primary aim with this example is to sketch out the relation between internal and external histories of a programme, but his example nevertheless lends itself to the present discussion.

It seems that many of the features seen in SIP do have historical antecedents, some of which can be seen in the content of Chapter 1. For instance, concept of "information processing deficits" might be understood as an echo of Goddard and Davenport's attribution of delinquency to "feeblemindedness" and other mental defects. It also borrowed assumptions about the role between frustration and aggression from earlier Drive theorists, and the notion of the "aggressive child" has roots in the concept of delinquency that arose in the early 20th century as psychiatric professionals sought mental illnesses within their curative power.

More compellingly, perhaps, one might tie SIP into the larger research programme of "cognitivism," which dominated American psychology research roughly from the 1960s through the 1990s. Cognitivism was marked by an effort to make psychology the study of the mind (as opposed to the study of behavior or the brain, as one tends to find in Behaviorism [1910s-1950s] or Connectionism [1990s-2000s], respectively), as well as a move towards anti-reductionism about the mind and an emphasis on innate cognitive structures. This movement has no shortage of antecedents in the history of ideas, including the work of Donders, Descartes and even Plato—importantly, neither do Connectionism or Behaviorism. It is not difficult to situate 20th century movements in psychology within the classic framework of Rationalism and Empiricism, which would place SIP in the context of a millennia-long rivalry between competing intellectual traditions.

One issue that complicates the above treatment of SIP is the sheer volume of possible classification schemes for a given research programme. SIP fits into a framework of Rationalism and Empiricism with reference to a subset of its content related features, such as its emphasis on cognitive architecture, the diminished role of learning mechanisms in SIP models, and so forth. However, one can also categorize SIP into any number of possible frameworks with reference to other aspects of its content.[115] For instance, one might fit

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[115] One can also use classification schemes that pertain to methodological features, such as the role that equations play in their theoretical structures, the complexity and number of their "core" assumptions, the stability of their auxiliary hypotheses, or even their

SIP theory into a classification based on conservativism, racial ideology, views regarding education, and so forth.[116] All of these are possible frames of reference that would potentially yield alternative structures to the history of ideas, and our preference for any particular framework is clearly historically and culturally contingent.

The lesson that I would like to take from this is that the multiplicity of possible frames of reference implies a multiplicity of possible research programmes that can describe a sequence of theories. This does not mean that any particular conception of research programmes is arbitrary,[117] but it is contingent on a cultural frame of reference, and consequently, so is a theory's apparent continuity with its predecessors. This means that Lakatos' model of continuity may be applied to SIP in many ways, and that one must accept a degree of relativism to a standard of continuity in doing so.

3.2.3 Hacking: Continuity as Stability

Hacking (1992) presents an account of stabilization in the laboratory sciences in which he addresses the "myth" of instability that arises in the work of Kuhn and Popper.[118] This myth, he argues, stems from the excitement generated by Einstein's groundbreaking work and the new developments in quantum mechanics during the first decades of the 20th century. Such achievements were anomalous, and biased subsequent philosophical analysis towards the notions of refutation and revolution. The conception of science as discontinuous, Hacking asserts, is linked to a lack of interest in the role of experimental science. Experimental science, relative to purely theoretical work, tends to be stable. Hacking provides three reasons

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correspondence to astrological events. However, given Lakatos' focus on content alone, this may not be an issue.

[116] E.g. perhaps SIP can be seen as continuous with Behaviorism, 18th century Deism, and Gnosticism under some alternative system of classification.

[117] One might potentially think of Newtonian mechanics as a set of twelve sequential research programmes, but we presumably have good reasons for thinking of it as one. That is not to say, however, that there are not good reasons for thinking about it in terms of twelve programmes.

[118] Hacking (1992) Pg 38

why science may *seem* stable, and suggests why that might actually be the case.

The first reason why science may seem stable is the scientific "habit of anachronism," through which scientists appropriate previous researchers' terms without regard for their original meaning. This certainly holds true for SIP, which used terms like "aggressive" and "maladjusted" that had rich research histories before Dodge ever published. In terms of *what* Dodge was measuring with peer popularity and teacher ratings in 1980, there is no obvious connection to the research conducted by earlier social learning theorists, neuroscientists, or psychoanalysts apart from the use of the term "aggression."

Second, scientific activity occurs as the interaction between a large set of traditions, and a discontinuity in one does not necessarily impact the others directly. In particular, theoretical discontinuities, as we see in Einstein's work, for instance, might leave traditions pertaining to the construction and use of instruments, analyzing data, and so forth relatively intact. In this model, many of the events that might result in incommensurability exist within largely continuous sets of traditions. This seems like a strong characterization of the apparent continuity in SIP research. While the formal aspects of the models shifted over time, other elements tended to hold fast. Occasionally multiple elements would shift together, presumably due to outside influences, as we see around the year 2000 when changes in the model, citation patterns, empirical standards, research questions and methods correlated with changes in youth crime rates and general changes in spoken and written English at the population level.

Third, scientific instruments tend to have their theoretical assumptions built into them. A scientific instrument, Hacking argues, is the material encoding of implicit, preestablished knowledge. Many of the instruments in SIP research occur in the form of standardized questionnaires or behavioral coding manuals. While technologically unimpressive, they are certainly laden with prior theoretical knowledge. Dodge and his colleagues designed and honed standardized instruments for measuring human behavior over decades, and devoted numerous studies to refining their statistical reliability.

Underlying the three aspects of continuity discussed above, Hacking identifies a possible root scientific stability—theories and instruments evolve together until an effective domain of relevant data is determined. These pairings of theory, instruments, and the appropriate domain of data effectively form a closed system. New theories are needed only when data arises that does not fit into an established closed system of theoretical knowledge and instrumentation. New theories can thus be more accurate than older ones, or reach deeper into the microstructure of reality, but they do not replace them in any real sense or result in discontinuities. There are strong reasons to doubt that SIP research constitutes a closed system, and I will discuss that topic in the subsequent section.

3.3 Maturity

One theme that arises frequently in the philosophy of science, often implicitly or without adequate discussion, is the notion of scientific "maturity." Prominent writers such as Gould (1990)[119] and Putnam (1978)[120] have made occasional use of the term to a variety of ends,[121] but I will focus entirely on the notion of "maturity" as it relates to specific models of scientific development. I will focus on the question of when—if ever—a research program "grows up" to become full-fledged science.

Scientific traditions exhibit a wide range of variation. Some scientific traditions are more prototypically "scientific" than others—consider, for instance, the apparent distance between contemporary astrophysics and hermetic astrology, or between linear accelerators and personality questionnaires.[122] Inevitably, most models of scientific content, rationality or activity exhibit profound biases towards prototypical scientific traditions, and have difficulty accounting for the patterns observed in other fields. Not unlike the classical notion of a demarcation criterion, which separates science from pseudoscience, many philosophers use the notion of "maturity" to separate more prototypical sciences from less prototypical ones in an attempt to define an ideal scope for their models of scientific development. In this section, I will explore conceptions of "maturity" as they arise in the work of prominent philosophers of science, and assess their applicability to SIP research.[123]

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[119] Gould (1990) pg. 172

[120] Putnam (1978) pg 21, 66.

[121] Gould uses the term to describe a particular conceptual shift in the study of the Burgess Shale, and Putnam uses it to delimit sciences about which one may be a "realist." Putnam draws from an unpublished essay by Richard Boyd.

[122] When I describe a tradition as being prototypically "scientific," I mean it in a cognitive sense—when one thinks of scientific theories, some stand out more than others as good examples. Psychologically, "science" is no different from any other concepts; robins are more "birdlike" than penguins (granted that people in Antarctica may develop the opposite impression), and for my purposes, Newtonian physics is the robin of the sciences.

[123] I will not evaluate models on the basis of whether they correctly identify the level of maturity exhibited by SIP theory. Given that SIP theory is somewhere on the spectrum between mechanics and

3.3.1. Kuhn: Maturity as Dogma

While Kuhn never discusses "maturity" outright in *The Structure of Scientific Revolutions,* his model of scientific development is often interpreted as requiring such a notion. Hacking (2002) makes this claim, and equates the moment of maturation with the development of a matrix-creating exemplar.[124] Hoyningen-Heune (1993) echoes this reading, by describing the transition from pre-normal to normal science as maturation.[125] In his discussion of Kuhn, Larvor (1998) similarly describes sciences ruled by disciplinary matrices as "mature." In his characterization, "immature" sciences are defined by 1) competing schools, 2) freedom to commit to metaphysical positions, 3) debates about foundational issues. "Mature" sciences, on the other hand, are marked by 1) enduring scientific communities and 2) puzzles for the community to work with—essentially, the existence of a matrix.[126]

Kuhn's occasional uses of the term "mature" reinforces these readings, as when he remarks that "the mature scientific community works from a single [disciplinary matrix], from a closely related set,"[127] or when he makes the stronger claim that the mature scientific community indoctrinates its members as if they were characters in Orwell's *1984*.[128] One potentially important difference, however, is that Kuhn seems to describe *communities* as maturing when he does use the word (sense A) whereas his commentators generally interpret maturation as applying to the level of disciplinary matrices (sense B).[129] Neither sense separates SIP from the more prototypical sciences, although both would suffice in separating it from less prototypical ones (e.g. psychoanalysis). I will now discuss both notions of maturity as they relate to the history of SIP theory.

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astrology in terms of appearing scientific, I will not assume that it is obviously mature or immature.

[124] Hacking (2002) Pg 87

[125] Hoyningnen-Heune (1993) pg. 27

[126] Larvor (1998) pg. 39

[127] SSR pg. 162

[128] SSR pg 167.

[129] This distinction is not perfect, as Kuhn was not particularly consistent in his vocabulary. For instance, on pg. 10, Kuhn seems to use the word "mature" to describe theories rather than communities.

The first sense of Kuhnian maturity encounters some difficulty in its application to SIP research. To begin with, I would like to distinguish indoctrination from mere training, the difference being that "indoctrination" suggests a more radical change in the student's reasoning. When Kuhn describes indoctrination, he presumably draws from the prototype of physics, in which students learn systematic and often deeply counterintuitive norms of thought. SIP theory, on the other hand, is largely characterized by extensions of ordinary reasoning. While Dodge did train numerous graduate students and post-doctoral fellows to become SIP researchers, there was hardly any room for indoctrination. It is true that SIP researchers are trained to use certain rules of inference, but many of these are general statistical principles that are hardly specific to SIP theory. The lack of systematic indoctrination would seem to indicate one of two things—that the SIP community is immature, or that SIP consists in multiple disciplinary matrices in separate but related fields. The former possibility conflicts with the fact that we have compelling reasons to attribute a disciplinary matrix to SIP researchers—namely, that they are working towards a common set of scientific problems, and doing so with common theoretical constructs, rules of inference and so forth. The depiction of SIP theory as a set of multiple distinct disciplinary matrices does not seem to match the patterns of close collaboration, co-authorship, and cross-disciplinary synthesis seen throughout the history of SIP research, and particularly in recent years.

The second sense of Kuhnian maturity also encounters trouble, as SIP theory does not conform to the picture of maturity painted by Kuhn's commentators. Regarding Larvor's criteria, SIP would easily qualify as mature (by all appearances, its community is enduring and has puzzles to solve), but it also fits the description of an immature science. That is, SIP researchers compete with numerous other schools of thought for funding and active acceptance, and occasionally address foundational issues (e.g. Dodge's writings on the relationship between "nature" and "nurture"). SIP researchers have moved beyond foundational issues for the purposes of their own scientific work, but they nevertheless exist within a competitive and pluralistic academic environment where they must periodically engage in the activities thought to be absent from mature science.

I do not believe that Hacking and Hoyningen-Heune's remarks pertain to SIP in particular, for SIP was borne through the mixing of pre-established disciplinary matrices and did not undergo a transition from pre-normal research to normal science. At a somewhat larger scale, however, both aggression research and mental-health research seem to have undergone such changes. As Chapter 1 demonstrates, the phenomenon of juvenile misbehavior likely predates writing, but it was not until the last century that it was studied in any a systematic way. This transition to "normal" science, however, did not involve a single "exemplar" of successful research. Rather, it occurred through numerous events, few of which were "achievements"—these includes the gradual realization of psychiatric impotence and the subsequent focus on relatively solvable problems, the popularity of left-wing politics among young mental health professionals, the appropriation of knowledge from other fields, and the growing prestige of "science" during the first decades of the 20th century.

3.3.2. Lakatos and Metaxopoulos: Maturity as Systematicity

As with Kuhn, Laktatos does not discuss maturation with rigor or detail, although his account of scientific change seems to require it. He states, "My account implies a new criterion of demarcation between 'mature science,' consisting of research programmes and 'immature science,' consisting of a mere patched up pattern of trial and error… Mature science consists of research programmes in which not only novel facts but, in an important sense, also novel auxiliary theories, are anticipated; mature science—unlike pedestrian trial-and-error—has 'heuristic' power."[130] Laktatos seems to have something in mind roughly along the lines of what Kuhn gestures at—maturity marks the point at which a theory reaches a degree of systematicity such that it fits within his domain of analysis.[131] Metaxopoulos[132] and Laudan[133] offer similar accounts of maturity within the Lakatosian framework.

Metaxopoulos provides a more specific articulation of Lakatosian maturity by naming three necessary conditions: 1) systematic construction of propositions and rigorous inferential rules (mathematization and axiomatization are the 'highest' expression of systematicity)[134] 2) procedures of empirical control, 3) pluralistic

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[130] Lakatos (1980) pg 87

[131] Along these lines, Lakatos pg 179 states that "the research programme is the unit of mature science."

[132] Metaxopoulos (1989) pg 204

[133] Laudan (1978) pg 150

[134] One immediate objection this condition is that it is far too narrow. Clearly tailored for physics (especially Newtonian mechanics), it is not a terribly accommodating for other well-established sciences such as

'conflictuality' between alternative research programmes.[135] SIP theory easily meets the second two criteria, but it falls somewhat short of the first. While there is some degree of conceptual systematicity in SIP, it does not seem to be the type of mathematically rigorous structure that Metaxopoulos has in mind. This might change with future developments, given the increasing mathematical sophistication of SIP modeling, but mathematics has of yet to become a substantial part of SIP's heuristic process. While SIP theory does seem to fit Lakatos' general description of a research programme at least roughly, Metaxopoulos' criteria would suggest that, at most, SIP research is in the process of becoming one.

This highlights a substantial shortcoming of the Lakatosian framework—it has little to say about "immature" science, even when it is quite close to maturity, or for that matter, about the transition from immaturity to maturity. Aside from SIP, numerous other fields of research, particularly in the life sciences, fall short of rigorous systematicity. It seems as if both Lakatos and Metaxopoulos have Newtonian physics in mind as the prototype of science,[136] and perhaps assume that the life sciences should (or perhaps do) strive to become more like Newtonian mechanics. One problem with this is that it hardly does justice to the plurality of epistemic approaches taken by the sciences. Similarly, it excludes a tremendous number of undeniably scientific research traditions from the proper scope of analysis. Moreover, some of the examples that appear in Lakatos' own writing (e.g. the Copernican Revolution) do not involve sophisticated systematic theoretical frameworks. This is deeply problematic for Metaxopoulos' narrow view of maturity, and more liberal criteria might not succeed in discriminating between more or less mature fields within the domain of Lakatosian analysis.

The essential problem for Lakatosian maturity is that it effectively marks any immature science as un-scientific.[137] In conflating maturity with the demarcation criterion, one puts

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chemistry. I will discuss this point in greater detail later in this section.

[135] Metaxopoulos (1989) pg 205

[136] Newton's laws of motion seem like the classic example of mathematization in a "maturing" science.

[137] While Lakatos does not directly state that maturity is a demarcation criterion, Laudan reads him in this way, and Lakatos' few remarks regarding maturity are consistent with this reading.

immature science in the same category as pseudoscience or non-science. This simply does not to justice to the range of existing scientific traditions. While SIP may be (or appear) less scientific than physics, it is dramatically more scientific than psychoanalysis, and any concept of maturity ought to be capable of explaining this.

3.3.3. Laudan: "Maturity" as a Ploy

In *Progress and its Problems*, Laudan anticipates many of my worries with Kuhnian and Lakatosian notions of mature science, and goes as far as to dismiss their conceptions of it entirely. His critique of Kuhn and Lakatos' concept of "maturity" consists in the following points:

1. It implies that sciences undergo permanent transitions from immaturity to maturity
2. There are no specific cases that exemplify maturity
3. There is a questionable implication that maturation is progressive
4. The concept is methodologically suspect, as it allows the builder of any model of scientific rationality to dismiss as irrelevant any historical counter examples.

I believe that these criticisms ring true, and that the fourth resonates particularly strongly with SIP research. Neither Kuhn nor Lakatos' models are capable of accounting for SIP in a wholly compelling manner, nor there is no prior reason to suspect that this would be the case. It is a matter of brute fact that SIP is recalcitrant to both frameworks of analysis with regard to maturity, and the relevance of this case to the evaluation of a model should not hinge entirely on a vague notion of "maturity."

3.3.4. Foucault: Structured Immaturity

In addition to addressing direct criticisms of the Kuhnian and Lakatosian models of maturity, I would like to briefly touch upon a vastly divergent characterization of science in history—one that I will ground in my reading of Foucault's *The Order of Things: an Archeology of the Human Sciences*.[138] Whereas both Kuhn and Lakatos direct their models towards only "mature" sciences and have little to say regarding scientific "immaturity," Foucault focuses entirely on historical cases in the human sciences that would almost certainly qualify as "immature" by any of the above-discussed criteria.[139] More importantly, however, whereas Lakatos sees immature science as the lack of theoretical systematicity and Kuhn sees it as the lack of a comprehensive disciplinary matrix, Foucault sees regularities and systems of thought. That is, structure and coherency exist within epistemic frameworks regardless of their degree of scientific maturity. For example, Foucault identifies particular intellectual systems that fostered highly contingent ideas, such as the continuity of nature during the 18th century[140] or the absence of a general notion of "life" in the Classical world.[141]

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[138] My interest is in the appropriation of certain ideas from Foucault's work. I do not commit to my interpretation being a particularly faithful reconstruction of his views or intentions as an author.

While never rigorously defined, these systems of thought are presumably comparable to those discussed by Kuhn and Lakatos as disciplinary matrices or research programmes. Drawing from Foucault, Hacking (2003) describes the structure of immature science as follows: 1) immature sciences are marked by regularities for which the hypothetico-deductive model is irrelevant, 2) these regularities determine systems of possibility of what counts as true or false, grounds for assent or dissent and what arguments and data are relevant.[142] This notion of maturity does not function as a demarcation criterion, nor does it explain how a theory might mature. On the contrary, it undercuts the motivation for any sharp distinction between mature and immature sciences, and it does so by attacking the myth that the existence of conceptual structure and internal logic sets the "noble" prototypical sciences apart from other research traditions.

Without touching the topic of maturity, my question is this—does SIP theory fall under the type of conceptual structure that Foucault ascribes widely to the life and behavioral sciences, or does it occur in a state closer to conceptual anarchy, a morass of inconsistent ad-hoc theory and facts? It seems obvious to me that the former holds true, and that SIP theory contains, or perhaps is contained within a larger system of thought. Aside from the standardized methodologies of the SIP research program, we see a similar type of standardization with regard to some of the theoretical content of the SIP tradition. For example, nearly all of the heuristic models within SIP theory rely on common assumptions about the core computational analogy, especially with regard to the discreteness and sequentiality of processing stages. In fact, neither discreteness nor sequentiality are necessary properties of an information-processing model, and there are countless other types of models that the researchers could have appropriated at any given point in the research program.

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[139] Pg. ix. Foucault directs his attention towards disciplines that concern "living beings, languages and economic facts"—as contrasted with the "noble" sciences of mathematics, cosmology and physics.

[140] Foucault (1970) pg 147

[141] Ibid. pg. 166.

[142] These two points are taken from a discussion of six hypotheses about Foucault, Hacking (2003) pg 95.

The reason why other types of models were not used, I imagine, is that much of SIP theory is unified by a common epistemic norm: the type of explanation that matters is that which tells us "where" the problem is. That is, SIP theory often treats explanation as a matter of "mapping" out the location of a problem in the mind's architecture, as opposed to reduction or a strictly causal account—either of which could presumably be modeled in terms of information processing in other ways. There are many other such norms in SIP research, and they determine which types of data researchers look at, which types of explanations count, and which conclusions are appropriate to draw. It is also worth noting, however, that this is not a closed system, and these regularities appear to be at least partially contingent on a number of external factors, as we see throughout the second chapter of this paper.[143] Foucault's notion of conceptual systematicity describes something important about SIP theory, regardless of whether it qualifies as "mature." This does not necessarily conflict with Kuhnian or Lakatosian notions of "maturity," but it does highlight the fact that such a distinction ignores important factors about SIP theory that might be more important for understanding its development over time.

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[143] One might suppose that historical and contextual factors are irrelevant to the harder sciences (Laudan pg. 150 attributes such a belief about mature sciences to Lakatos), but this is not likely to be the case. Much contemporary work in physics is funded by the military, and a significant amount of medical science is funded by tobacco corporations. While these factors may not change the accuracy of the work being performed, they certainly shape the types of research questions that scientists ask and the types of answers that they seek.

3.3.5. Hacking: Maturity as Stabilization

Hacking (1992) offers an account of maturity that diverges from those discussed elsewhere in this section. Hacking does not use "maturity" to mark the boundaries of his model, nor does "mature science" receive any privileged position in his analysis. Rather, maturity occurs as a matter of brute contingent fact—instruments and systematic theories evolve around sets of data that may be explained by them, and sometimes reach a point where the limits of their applicability are clear.[144] At this point, the instruments and theory are paired with a relevant data domain, and data outside of this set will simply be regarded as irrelevant. Such theories require the development of a new theory or new instruments, and this recombination might eventually yield yet another "mature" science. Hacking provides the example of geometrical optics, which has thoroughly "matured" relevant to the data set relating to rectilinear propagation of light, but which has little to do with phenomena such as blurred edges.

What is compelling about Hacking's sense of maturity is that it describes highly prototypical sciences (e.g. Newtonian physics) and also describes the process by which a scientific tradition becomes mature. Additionally, it is not vulnerable to Laudan's criticisms of Kuhnian or Lakatosian maturity, given that it neither commits to progress, permanence of maturity (after a period of stability, a science's data domain might increase in size with new discoveries), nor to the demarcation of the proper domain of philosophical analysis.

It seems highly unlikely that SIP theory is mature (SIP researchers are still finding new problems to solve), but Hacking's model of theory, instruments and relevant data domains seems descriptively accurate. The researchers' perennial search for statistically reliable categories and attempts to relate them theoretically seems to fit symbiotic model of evolution suggested by Hacking's model. In fact SIP presents an interesting case, where the data itself may shift to fit the experimental instruments. Specifically, many of the observed correlations between the categorization of children and their behavior may relate to 'looping effects,' a phrase that Hacking (2006) uses to describe phenomena in which classifications of people become self-fulfilling prophecies.[145] If this is the case, the reification of the "aggressive child" may have actually reinforced the reliability of the category. While this type of symbiosis is unlikely to occur in the laboratory sciences with which Hacking is concerned, it nevertheless presents a strong case for the mutual vindication of elements in a scientific tradition.

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[144] Hacking (1992) pg 55.

[145] Hacking (2006) pg. 286.

3.4 Progress

To many 20th century philosophers, "progress" lies at the very heart of science and sets it apart from all other intellectual pursuits. Unlike the notion of "maturity," however, few contemporary historians or philosophers of science have been willing to take it wholly for granted. "Progress" has two basic components: the descriptive claim that change occurs and the normative claim that this change constitutes an improvement. One might also distinguish between different aspects of progress: economical (increases in funding), professional (rising status of individuals or institutions), methodological (refinement of methods and instruments), and cognitive (advancement of knowledge).[146] SIP theory clearly changed over the past three decades, and there is little question that it enjoyed substantial economical and professional progress, as well as some degree of methodological progress in the honing of its experimental instruments and statistical methods. The question of cognitive progress, however, presents a more ambiguous and philosophically interesting topic. In this section, I will discuss Kuhn, Lakatos and Laudan's view's regarding progress in the context of SIP theory.

It is worth noting that, going into this section, we might expect models of progress to identify SIP theory as progressive. This is for the simple reason that, purely on an intuitive, pre-theoretical level, the content of SIP theory seems to improve over time—it becomes more testable, applies to new cases, and explains things that previous theories could not. A compelling model of progress should either honor this intuition or provide a substantial rebuttal to it.

3.4.1. Kuhn: Fractured Progress

Kuhnian normal science is fundamentally a puzzle-solving enterprise, with progress as the "inevitable" result of finding solutions.[147] Progress occurs within a disciplinary matrix as puzzle solutions accumulate in number and increase in precision. Incommensurability prevents the meaningful comparison of two disciplinary matrices, which poses a serious problem for progress across revolutions—that is, how can improvement occur without comparative evaluations? This problem commits Kuhn to denying cognitive progress across scientific revolutions, but it also requires that he explain the apparent superiority of our own scientific explanations to those of ancient thinkers. At points, Kuhn seems to adopt a relativistic attitude, implying that developments that appear progressive at a particular point in time do so in virtue of the observer's disciplinary matrix.[148]

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[146] Niiniluoto (2002) refers to these categories.

[147] SSR pg 166

In other places, however, Kuhn seems to allow for a type of indirect progress across revolutions. Godfrey-Smith synthesizes Kuhn's position in terms of increasing efficiency and problem solving power over time[149]—that is, progress exists insofar as the number and precision of solutions increases. This is still a type of cognitive progress, but manages to escape the problem of incommensurability by focusing on structural aspects of a disciplinary matrix thought to be external to its semantics or theoretical content. While not inevitable, this sense of progress presumably characterizes long-term development of science in many fields, as there are few examples of ancient sciences that best their modern analogues in terms of precision or the number of problems solved.

The history of SIP theory does not support the claim of cumulative internal progress, nor does it support the claim of purely structural external progress. As SIP became increasingly empirical, statistical methods increased in sophistication and the concept of the "aggressive boy" subsided, the newer findings effectively *replaced* many of those from the 1980s and 1990s rather than adding to them. We can see this in the fact that the findings surrounding "aggressive boys" (as measured by peer popularity ratings and teacher comments) lost their explanatory role once the researchers found similar findings with more measurable and objective categories (e.g. physically abused children). Few papers after 2000 cite earlier SIP research aside from a few classic studies that typically get re-framed with the vocabulary of newer research questions (e.g. Dodge 1980 and Crick 1994). Moreover, entire heuristic models were periodically recast or fully discarded, as we see with the ill-fated models from 1984 and 1986. Even the development of an ontological model of cognition was abandoned in favor of a more instrumental approach in Fontaine and Dodge's research.

Regarding cross-matrix progress, SIP appears to have inherited a great deal of knowledge from other disciplinary matrices—in particular, the assumptions underlying hostile attribution biases are attributed directly to the contributions of earlier drive theorists, and the conceptualization of cognition has clear roots in earlier cognitive science research. Similarly, the puzzles of SIP theory were not newly created in the wake of Dodge's 1980 paper, but have deep roots in earlier theoretical and experimental approaches. It is simply not the case that knowledge accumulates progressively in SIP theory, nor is it the case that it started anew as a framework for problem solving.

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[148] SSR pg 163.

[149] Godfrey-Smith, pg 95.

3.4.2. Lakatos: Progress as Growth

In Lakatos' model, progress does not occur at the level of particular theories, but emerges across the sequence of theories that compose a research programme. Each theory within a research programme arises by adding auxiliary clauses to (or semantically reinterpreting) the preceding theory in order to account for previously unexplained information. Thus, each new theory within a research programme has at least as much content as the un-refuted content of its predecessor. A series of theories is "theoretically progressive" if each new theory has excess empirical content over the theory before it—that is, if it makes new predictions.[150] If some of this new empirical content is corroborated, and that theory leads to the actual discovery of new information, then the series of new theories qualifies as "empirically progressive."[151] A problemshift[152] is progressive if it is both theoretically and empirically progressive; if either condition is unfulfilled, the problemshift is *degenerating* and it is considered "stagnant" when discoveries outpace theoretical growth.[153] A degenerating problemshift (even one that reinterprets previous theory without adding empirical content) crosses the line of scientific demarcation and becomes mere pseudoscience.

By Lakatos' criteria, it seems as if only a subset of internal developments within SIP research were progressive. Among these, one might include the perennial application of SIP theory to new categories[154] (e.g. neglected children, depressed children), which certainly constitute expansions of its empirical content, and were generally corroborated by varying degrees of statistical reliability in those categories. The mathematical developments of the 2006 RED model have yet to yield new discoveries, but seem promising. On the other hand, many of the more dramatic internal changes seem to have done little to directly expand SIP theory's empirical content. For instance, while the 1986 model led to experimental modifications, it did not lead to new predictions. Similarly, the 1994 model seems to have made SIP theory more appealing to outside researchers, but did not appear to generate new content. A rather substantial portion of the changes within SIP theory did little to increase its empirical content, which puts it somewhere on the border of science and pseudoscience.

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[150] Lakatos (1978) pg. 33.

[151] Ibid. pg 34.

[152] Lakatos uses this term to describe the transition between sequential theories

[153] Ibid. pg 112.

It is worth questioning Lakatos' depiction of empirical growth as the sole determinant of progress. For instance, the 1994 model made SIP research compatible (and possibly reducible) with the much larger research programme of Connectionism, which hardly seems like an unscientific theoretical shift. Moreover, some of the important theoretical changes involved the loss of information, and more importantly, the loss of empirical content. Consider the growing empiricism and movement away from the "aggressive boy" after about 2000—this shift resulted in the "loss" of the empirical content gained through SIP's earlier studies. That is, newer experimental findings often replace their predecessors with SIP theory without necessarily resulting a net growth of content. The newer results were often more theoretically sound than their predecessors (e.g. eliminating sources of semantic or experimental circularity) or statistically reliable. This deeply problematizes Lakatos' emphasis on growth to the exclusion of more subtle types of change. These cases make it clear that the "quantity" of empirical content alone does not provide a compelling account of scientific progress.

3.4.2 Laudan: Non-Cumulative Progress

Similar to Kuhn, Laudan envisions scientific progress within a research tradition as a matter of problem solving. The adequacy or effectiveness of a theory is a function of the number and significance of the problems that it solves, and the progress (or rate of progress) is a function of increasing or decreasing effectiveness. Laudan looks at two measures of progress: 1) the general progress of a research tradition, which is determined by comparing the adequacy of the sets of theories which constitute the oldest and the most recent versions of a research tradition, and 2) the rate of progress of a research tradition, determined by the changes in the momentary adequacy of a research tradition during a particular time span.[155]

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[154] Obviously, this does not pertain to cases where one category replaces another, as with "aggressive boys" and "abused children."

Like Kuhn, Laudan envisions science as a process filled with discontinuities, at least to the extent that the domain of solvable (and solved) empirical problems tends to shift as research traditions fall in and out of prominence.[156] Any scientific revolution is likely to bear a cost in the store of accumulated solutions to problems, but the lesson that Laudan takes from this is simply that any theory of scientific development must leave room for progress in certain cases where the "problem domain" constricts.[157] In cases where problem domains do not fall within one another, the solution is simply to consider not only the number, but also the *significance* of the problems at stake and weight them accordingly. Thus, it is possible to make measures of theoretical effectiveness and the rate of progress despite apparent differences in the semantics of two theories.[158]

Assuming that SIP theory is scientific (and therefore progressive), Laudan's view fares well in characterizing the relationship between SIP theory and earlier research traditions. Many research traditions had approached the topics of aggression and delinquency prior to SIP theory, including decades of accumulated work in related fields such as drive theory, social learning theory, deindividuation theory, and so forth. While SIP theory is largely continuous with various predecessors in terms of major research questions or assumptions about human thought and behavior, it also *lost* many of the solutions to empirical and conceptual problems that had been conquered by them. Social learning theory and drive theory had been particularly successful in describing the circumstances under which aggressive behavior could be learned and provoked respectively, but SIP inherited few solutions to these problems. Rather than build on previous approaches, Dodge and his colleagues simply ignored issues regarding learning or provocation except insofar as they concerned cognitive architecture. Eventually, they built their framework outwards to regain some of the lost problem domain in an ad-hoc manner (e.g. Dodge's 2004 on the topic of the "nature vs. nurture" debate), but these efforts never reached the topical sophistication of other traditions that had focused on them entirely.

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[155] Laudan (1978) pg 107.

[156] Ibid pg 149.

[157] Ibd pg 150

[158] Although his solution to the problem of non-cummulative progress resembles that offered by Kuhn, Laudan rejects Kuhnian incommensurability on the basis of its implicit theory of meaning. See Laudan (1978) pg. 142.

With regard to the "general" progress within SIP theory, Laudan's model seems like a fair description of many theoretical changes. For instance, as the models used in SIP theory change over time, a substantial amount of problem domain is lost. In moving from an ontological model in 1986 to a more ambiguous model in 1994 to a wholly instrumental model in 2006, we see a tremendous loss of solutions (i.e. the loss of a "literal" explanation of information processing patterns) matched with what appear to be changing values within the research community. That is, much of the 1980s and 1990s saw rises in youth violence, and the researchers pursued a psychological solution to the murky etiology of delinquency. As time passed and social factors shifted, the importance of a "cognitive" answer waned and more empirically grounded, externally based causal explanations became a higher priority. The significance of problems changes over time, and by assigning weights to various aspects of the problem domain, it becomes possible to model the history of SIP as internally progressive. Each successive change was appropriate with reference to the research tradition at that particular time, and each improvement came with the risk of losing something that previously lay within the theory's explanatory power.

4. Lessons From SIP Theory

My first two chapters tell the history of SIP Theory, as well as the individuals, institutions and theoretical precedents that created it and propelled its development. In the previous chapter, I review four topics that have emerged across various efforts to model scientific development. For each topic, I use the history of SIP theory as a frame of reference, gauging the descriptive adequacy of the relevant philosophical models. Some models prove far more accurate than others—Laudan, Foucault and Hacking, for instance, offer models that successfully characterize a number of patterns that arise in the history of SIP theory. Kuhn, Lakatos, and Metaxopoulos do not generally succeed in this regard. The central question of the present section is this: what can we learn from the failure of these models to describe SIP theory? In addressing this question, I will bring this paper to its concluding remarks.

4.1. Methodologies Matter

Perhaps the most striking feature that divides the philosophers noted above is the range of their methodological attitudes. To say nothing of the role that normativity plays in each model, the styles of reasoning that underlie their descriptive claims tend to fall into two categories. Foucault, Hacking and Laudan tend to rely on *bottom-up* methodologies, basing their generalizations in a diversity of historical examples (and rarely relying on "classic" examples from the history of science), whereas Kuhn, Metaxopoulos, and particularly Lakatos tend to use a *top-down* approach, appealing more to logic and argumentation than to particular cases. The range of top-down and bottom-up models forms a spectrum, so the binary division of theories here is, at most, a coarse one. Although this sample size is certainly not sufficient to justify a categorical judgment against top-down approaches, it certainly seems as if strongly bottom-up methodologies demonstrate superior descriptive reliability in the case of SIP theory.

There are a few possible explanations. One is that the bottom-up approach is simply more accurate, for the simple reason that inductive reasoning *usually* works. Approaches that rely less heavily on induction, while perhaps more clearly reasoned or logically rigorous, are less likely to capture the typical patterns observed in history. A related reason is that top-down approaches tend to be more vulnerable to ideological influences or intellectual idiosyncrasies in virtue of their detachment from a common historical frame of reference. Consider, for instance, that Kuhnian incommensurability stems from an unusual (and perhaps naïve) theory of semantics rather than any concrete historical cases.[159] Similarly, Lakatos uses the Tarski-Popper notion of "content," which certainly did not arise through the study of historical cases.[160] Perhaps more importantly still, top-down approaches simply use less information to inform their models, which comes at a cost in veridicality. Along these lines, perhaps top-down models focus more on fewer features, which results in relatively distorted representations of scientific knowledge.[161] Another possibility is that each model succeeds or fails by its own measure, and that no generalization can adequately capture what makes for an accurate model. At least with regard to the models discussed in this section, however, there is little question that the bottom-up approaches describe SIP theory far more accurately.

One might worry that top-down models tend to be highly normative, whereas bottom-up models tend to de-emphasize the evaluation of the traditions that they describe. If this is the case, then descriptive accuracy is not an appropriate criterion of appraisal for top-down theories. In fact, a normative theory need not be top-down,[162] but the objection nevertheless remains that descriptive accuracy is not an appropriate measure of a normative model. This means trouble because top-down models are generally normative. I have two replies to this objection.

First, top-down approaches continue to encounter serious problems, even if one discounts descriptive accuracy as a criterion of appraisal. It seems reasonable to hold that a normative model should identify clear cases of when a particular field is good, or progressive. Yet, even this task proves disastrous for the highly top-down approaches discussed in this chapter. For instance, Lakatos and Metaxopoulos successfully identify certain improvements in the history of physics (e.g. the Newtonian revolution), but their success with regard to other (presumably) good or improving scientific traditions is open to considerable doubt. Consider, for example, how these models would apply to the history of chemistry, biology, or the behavioral sciences. It is not difficult to see that they lack the sensitivity to identify even substantial improvements in less "prototypical" fields, from the Darwinian revolution to the shifts within SIP theory.[163] Unless one is willing to maintain that the only good scientific changes are those that make a theory more like Newtonian mechanics, one must recognize that these accounts are left wanting.

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[159] Laudan (1978) pg 142 makes this point.

[160] Laudan (1978) pg 77.

[161] Ramscar et al. (2009) makes a strong case for an analogous view with regard to cognitive representation in discrimination learning.

[162] Laudan's model is both normative and bottom-up. It is also relatively accurate descriptively.

Second, it is not appropriate to reject every type of descriptive accuracy as a measure of normative models. Apart from identifying good research programs, I would like to propose an additional, related criterion of appraisal: descriptive accuracy in *cases of good or improving research programs*. Put simply, a model of scientific progress should describe scientific progress accurately, even if it does not suffice as a more general description. This criterion stems naturally from my reading of Kuhn, Lakatos and Laudan, all of whom go far beyond the point of merely appraising scientific change, and attempt to account for *how* scientific progress occurs.

Much like the first criterion, the second one creates a problem for top-down normative approaches. The model advocated by Lakatos and Metaxopoulos encounters numerous complications when applied to SIP theory, and it is similarly cumbersome with regard to numerous research traditions in the historical and life sciences. Laudan's normative model, on the other hand, which derives from a more distinctly bottom-up methodology, offers a far more compelling description of SIP theory. Not only is descriptive accuracy an important part of the appraisal of normative models, but it reinforces the argument against top-down normative models of scientific development.

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[163] Perhaps Lakatos would conclude that Newtonian mechanics is simply a *better* (or better-developing) science than the study of genetics, nutrient cycling, or psychological ailments. While perhaps theoretically motivated, this position does not do justice to the plurality of scientific norms. What works in one field may not work elsewhere, and one should question the value of any model that privileges the methods or concepts of one field over every other.

4.2 The Limits of the Epistemology of Science

In terms of the actual content of the models, the most striking commonality across the less successful ones is their inability to account for one feature in particular: *the loss of information over time*.

Pera (1989) wrote disparagingly of the “Cartesian Syndrome” that dominates many discussions in the philosophy of science—the notion that science is an intellectual activity consisting of the accumulation of knowledge and submitted to universal rules, the violation of which leads to irrationality.[164] This conception of science, as the accumulation of systematic knowledge through rigorous and rational means, evokes the Cartesian tree of knowledge, with its disciplinary branches growing outwards from sturdy and indubitable metaphysical roots.[165] Pera argues that Lakatos in particular held such a view, and moreover, that many arguments against the continuity or rationality of science (e.g. the views of Feyerabend or those which are often attributed to Kuhn) hinge on the absence of systematicity across scientific traditions. The lesson that I would like to take from Pera is an obvious but important one: much contemporary philosophy takes place within a Cartesian epistemic framework devoted to the acquisition of justified positive knowledge.

Nearly all of the philosophers discussed in this chapter treat the study and analysis of science as a form of epistemology, focusing on the special type of knowledge that science is believed to produce. In studying the accumulation or net growth of problem domains, puzzle solutions, referential facts, corroborated empirical content and so forth, a philosopher takes part in the Cartesian epistemic tradition of positive knowledge. The problem with this approach is that the history of SIP theory, as well as science more broadly, is not the story of an indiscriminately growing body of knowledge and problem solving power, but of highly selective processes that reject or forget a tremendous amount of information over time.

As an alternative approach to traditional epistemology, Proctor and Schiebinger (2008) propose a new discipline: *agnotology*, or the study of ignorance. This includes the domains of scientific knowledge that are kept highly secretive by military or industrial interests, deliberate efforts to obscure or distort scientific knowledge (e.g. as with the tobacco industry and medical science), and the act of forgetting. Ignorance, when framed as more than the absence of knowledge, has the potential to reveal much about SIP theory that the classic philosophical models were largely insensitive to.

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[164] Pera (1989) pg 173.

[165] See the preface to Descarte’s Principles of Philosophy.

4.2.1. Applications of Agnotology: A Few Examples

One particularly effective application of agnotology is the use of citations in SIP theory. Citations fulfill a mnemonic role in the scientific literature, and the lack of citations is often as important as their inclusion. For instance, Dodge and his colleagues went many years without citing Huesmann (1988), who offered a rival SIP model to that proposed by Dodge in 1986. Once Dodge updated his model in the mid 1990s, however, he and his colleagues began to cite Huesmann (1988) as a representative older model—and sometimes excluding references to Dodge (1986). This strategic use of citations highlights the role that withholding information can have in crafting the "history" of SIP research that appears in literature reviews.

More substantially, SIP researchers generally do not cite papers more than a few years old, the exceptions being the classic papers like Dodge (1980) or Crick (1994). The vast majority of studies are quickly forgotten and their insights buried under the growing tide of publications; those that do survive in citations are often reframed in substantial ways. The fact that scientists do not explicitly appeal to or react to significantly older research is tremendously important for any model of scientific development, yet this topic does not appear in any of the work discussed earlier in this chapter.

It is also worth discussing the fact that SIP researchers rarely, if ever cite research from other fields. This type of selectivity is easy to take for granted, but it is crucial in understanding contingent constraints on the problem domain of a given research tradition. For instance, SIP research takes place largely independently from work in neuroimaging, hormonal research, genetic research, learning research and other relevant fields, and each of these fields is similarly isolated from the others. There is no theoretical reason for this division, but there is far more information than the SIP researchers (or those of any other field) can realistically ever learn, and the very practice of research requires a radical constriction of possible hypotheses. Such systematic ignorance is abundant in science, and does much to shape the development of contemporary research. Yet, none of the philosophical models discussed in this chapter address this issue.

The examples described above include only a few of the many potential applications of agnotology to the history of SIP theory, and presumably to other research traditions as well. Such considerations have been overlooked purely due to a contingent philosophical bias towards positive knowledge, and ought to play a greater role in philosophical models of science. Laudan's willingness to incorporate constricting problem domains into his model makes it far more compelling than those offered by Kuhn or Lakatos, but even his account does not describe the systematic ignorance and loss of information that occur through normal scientific practice.

4.2.2. Losing Explanations and Raising Standards

Earlier in this chapter I discuss cases where progressive internal developments in SIP theory involve a cost with regard to its empirical content or problem domain. In this section, I will argue that the loss of information (i.e. content, problem domain, facts) is an important part of raising empirical standards for SIP theory, and may play a similar role in other research traditions.

From 1980 through the present, the theoretical claims made by SIP researchers have extended far beyond the realm of available experimental evidence. During the 1980s and 1990s, this was particularly true.[166] For instance, the 1984 model of social feedback loops was only tangentially related to the cognitive experiments in the study. The 1986 model was appropriated from outside researchers, and bore little relation to the experiments that were used to support it. Moreover, of the five steps comprising the model, subsequent experiments only seem to have investigated two. Even the elaborate 1994 model was only indirectly relevant to the SIP experimental tradition. During this period, SIP theory also boasted an elaborate ontology of theoretical entities: in addition to the cognitive architecture outlined in the models, SIP theorists were committed to the existence of "the aggressive boy," as well as the "rejected child," the "controversial child" and so forth.

Roughly between 2000 and 2010, SIP theory became a more humble enterprise. While the models continued to grow in complexity, their ontological significance deflated to the point of undisguised instrumentalism. The superfluous steps of the models were ignored and efforts were made to focus on quantifying the relatively measurable "response decision" step. Moreover, the "aggressive boy" began to disappear from the theory, to be replaced by more objectively measurable categories. SIP researchers also began using data from long-running longitudinal studies that overcame many of the methodological shortcomings of their earlier research. Together, these changes constitute a substantial shift towards the use of higher empirical standards. What is more, these shifts involved shedding the unsupported weight left over from earlier versions of SIP theory.

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[166] I should clarify that there are some theoretical issues that are intricately tied to experimentation—especially notions of theoretical entities and their corresponding experimental categories. This topic is, however, largely implicit and not explicitly discussed *as theory* in the SIP literature.

While weakly supported by experimentation, I do not mean to suggest that the lost theoretical knowledge was of no value to the researchers. On the contrary, the SIP researchers surrendered a tremendous amount of explanatory power by adopting a more modest and robust approach. Dodge's 1986 paper earned him tremendous professional recognition and success, and not without reason. Namely, it provided a long awaited psychological account of a poorly understood and culturally important problem. Similarly, the 1994 model joined diverse research traditions to create a model with strong conceptual appeal across many fields. By giving up these theoretical constructs, SIP researchers gradually traded explanatory power for the promise of more predictive power and a more streamlined approach.

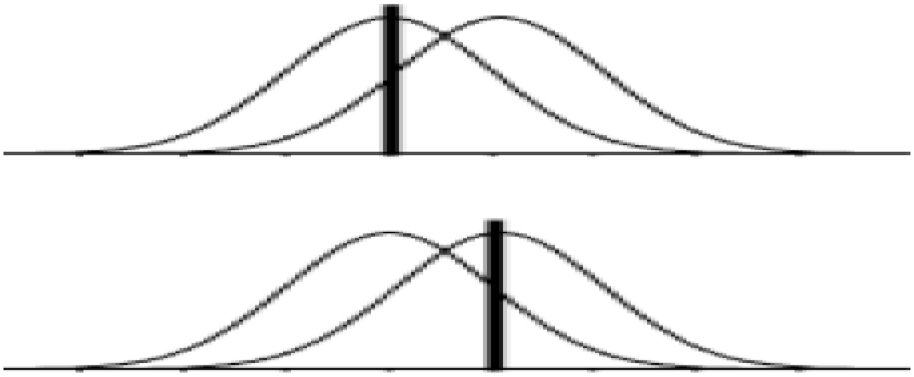

Figure 17 Raising a statistical threshold (shifting it to the right) lowers the number of both hits and false alarms (depicted as the total area to the right of the bar).

The notion that raising empirical standards entails a constriction of a theory's problem domain should not be surprising. Signal detection theory makes this clear enough: raising a threshold lowers the number of Type I Errors (false alarms, or in this case, false content) but also lowers the number of results reported as positive (in this case, content that can be believed). See figure 17. By its very nature, raising empirical standards increases a theory's selectivity, and in the case of SIP theory, there was quite a bit of conceptual excess.

Such epistemic sacrifices are to be found throughout the history of science. Explanation is an ancient feature of human thought, readily found in ancient mythology and religious doctrine; in rejecting Aristotelian cosmology, Galileo lost a wealth of explanations. Predictive power and empirical constraints on theory, however, are features most readily found in prototypical sciences, and rarely found elsewhere. These features are critical for the type of stabilization that Hacking describes as occurring in the laboratory sciences. It certainly seems as if, in a meaningful sense, SIP theory became more scientific as a result of its lost ground.

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